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ART. XV.—*On the Velocity of Light and the Sun's Distance;* by Prof. JOSEPH LOVERING, of Harvard College.

FOUCAULT'S recent experiment on the velocity of light, though of a less popular character than his celebrated pendulum experiment to prove the earth's rotation, will, nevertheless, attract even more attention among men of science. If its results are placed beyond doubt, they will affect astronomy to a degree not possible for the pendulum experiment, unless it had come as early as the time of Galileo. I shall examine Foucault's investigation on the velocity of light: 1, as it influences the science of Optics; and 2, as it tells upon one at least of the vexed questions in astronomy.

In the circle of the sciences, the centre may be placed anywhere and the circumference will be everywhere: such is the mutual dependence of each upon all the rest. After the science of optics has furnished astronomy with the telescope, the astronomer discovers with it the satellites of Jupiter and the aberration of light, and with the help of these phenomena assigns the value of the velocity of light, and thus repays to optics the debt incurred by his own special science. Now, for the first time, the science of optics has relinquished the guardianship of astronomy; ascertained by direct experiment one of its own fundamental data; and thereby, possibly, put astronomy under a new obligation, to be cancelled, doubtless, with interest, hereafter.

Let us glance first at the two astronomical methods of measuring the velocity of light. While the senses of touch and taste

act only by contact, those of hearing and seeing bring the mind into communication with distant objects. The air and the omnipresent ether supply the delicate and ever ramifying threads by which telegraphic intercourse is maintained with the ear and the eye. When the origin of the sound or the light is at a large distance, compared with the velocity of the acoustic or luminous wave, allowance must be made for the time taken by the news of an audible or visible event to come to us. Only the vast spaces of astronomy are commensurable with the great velocity of light, and furnish a sufficiently large theatre for a direct experiment upon it. But, in stellar astronomy, the magnificence of the extent of view so far transcends in magnitude even the velocity of light, that the luminous ray, vast as is its velocity, seems to loiter upon its long way.

Hence, in astronomy, a distinction exists between the *actual* interval of successive events and the *apparent* interval. For example, the first satellite of Jupiter revolves around its primary in about 42½ hours; and, therefore, enters the shadow of Jupiter, and is eclipsed, once every 42½ hours. As it takes light more than 40 minutes to pass over the average distance of Jupiter, the eclipse is not seen until so many minutes on the average after it has happened. If this delay were constant, the interval of successive eclipses would not be changed. But in the course of six months the distance of the earth from Jupiter increases by the diameter of the earth's orbit, and in the next six months changes back again; and when the earth is nearest to Jupiter, the news of an eclipse reaches us in about 32 minutes, whereas, if the earth is at the greatest distance, 50 minutes are required.

Consequently, the intervals between successive eclipses, as they exist for our eyes, are variable, being sometimes larger and sometimes smaller than the real intervals. This irregularity in the apparent intervals of the eclipses of the same satellite, at first attributed to errors of observation, finally conducted Römer, in 1675, to the discovery of the velocity of light. Delambre, after discussing 1000 of these eclipses, observed between 1662 and 1802, calculated the velocity of light to be such as to require 493·2 seconds to pass over the mean distance of the sun. If this time divides 95,360,000 statute miles, which is the sun's distance as given by the transits of Venus in 1761 and 1769, according to Encke's computations, the quotient, or 193,350 statute miles, is the velocity of light in a second.

The second process which astronomy has supplied for obtaining the velocity of light may be called the indirect method. It demands not a *space* but a *velocity* which is commensurable with the velocity of light. If two such velocities are compounded together, according to the principle of the *parallelogram of mo-*

tions, there is a resultant motion, the direction of which deviates sensibly from that even of the largest motion which enters into the composition. In nature, the velocity of the earth is compounded, in this way, with the velocity of light, and imparts to the light an apparent path differing by a small angle from the true path. The angular displacement which this causes between the apparent and real places of a star is called aberration, and was first discovered by Bradley in 1726: this astronomer explaining, on this simple principle, anomalies in observation which had hitherto been considered accidental. As the displacement of the star works opposite ways at opposite seasons of the year, half the difference between the extreme places is the distance from the apparent to the true place, or the constant of aberration. This, when known as an observed fact, establishes the ratio between the velocity of light and the velocity of the earth, and enables the astronomer to assign the value of the one with all the accuracy which pertains to his knowledge of the other. Accepting Struve's determination of the aberration, viz: 20″·45, the velocity of light is calculated to be 10,088 times as great as the velocity of the earth. The mean velocity of the earth is known with all the certainty which belongs to our knowledge of the magnitude of the earth's orbit: that is, of the sun's distance. Assuming, as before, that the distance derived from Encke's parallax is the most reliable, the velocity of the earth in one second of solar time is 18·977 miles. This multiplied by the aforesaid ratio gives 191,513 miles for the velocity of light by Bradley's principle. It appears therefore that the velocities by the two methods of astronomy (the direct and the indirect) differ by 1837 miles; a small quantity comparatively, being only *one per cent* of the whole velocity. Whatever other value is adopted for the sun's distance will alter these two results proportionally, without disturbing the ratio between them. I may add that the velocity which aberration ascribes to light belongs to it at the earth's surface; that is, in the dense atmosphere: whereas, the velocity discovered from the eclipses is that which extends through the planetary spaces. This distinction, however, will do little towards bringing the two results into greater accordance. The velocities of light in different media are proportional to the indices of refraction inversely: which in the case presented are as 1 to 1·000294. This theoretical difference of velocities is less than $\frac{1}{5000}$ of the whole, or less than 70 miles.

Compare with these conclusions of astronomy two experimental results on the same subject. Although Wheatstone's experiment on the velocity of electricity, published in 1834, suggested the possibility of measuring, in a similar way, other great velocities, I shall consider first a contrivance of Fizeau,

equally applicable to light and to electricity. If a wheel finely cut into teeth on its circumference is put in rapid rotation, a ray of light, which escapes between two consecutive teeth, will, after being reflected perpendicularly by a mirror, return to strike the wheel at a different point, and either be intercepted by a tooth or admitted at another interstice. Suppose the velocity of the wheel just sufficient to bring the adjacent tooth to the position whence the ray first started, in the time which the light occupies in going to the mirror and returning. In this time the wheel has moved over an angle found by dividing 360° by twice the number of teeth which the wheel contains. Therefore the time taken by light, in going over a line equal to twice the distance of the mirror, is that portion of a second found by dividing unity by the product of the number of turns the wheel makes in a second, multiplied by double the number of teeth on the wheel; the velocity of the wheel being first made the smallest which will cause it to intercept the light. Such an experiment was made in 1849 by Fizeau, the wheel being placed in a tower at Suresne, near Paris, and the mirror upon a hill (Montmartre) at the distance of 8633 metres. As the wheel contained 720 teeth, and the slowest velocity which produced obscuration was 12·6 turns a second, it appeared that light required $\frac{1}{181\,44}$ of a second to go 8633 metres and return. Hence its velocity was 313,274,304 metres or 194,667 miles a second. The French Academy thought so favorably of this attempt that they referred the subject to a scientific commission consisting of Biot, Arago, Pouillet and Regnault, with authority to procure a grand machine for repeating the experiment.

When Arago advocated the claims of Wheatstone to the vacant place of Corresponding Member of the French Academy in the section of Physics, it was objected that Wheatstone had only made a single experiment without having discovered a principle. Arago engaged to prove that the candidate had introduced a fertile method of experimentation which would be felt in other sciences as well as electricity. For example: the corpuscular theory of light requires that the velocities of light in different media should vary directly as the indices of refraction, whereas the undulatory theory inverts this ratio. Arago prepared for the trial by experiments on rapid rotation, the mechanical difficulties to be overcome, and the comparative advantage of slower rotations assisted by several reflexions, in place of a single mirror turning with its maximum speed. Aided by the refined skill of Breguet, he realized velocities in the mirror of 1000 turns a second, and of the axis, detached from the mirror, of even 8000 turns. In the meanwhile his eyesight began to fail him, and younger physicists entered into the fruit of his labors. After Foucault and Fizeau, by separate efforts, had de-

cided the question, in relation to the velocities of light in air and in water, in favor of the undulatory theory, and thus confirmed a conclusion which Arago reached by *diffraction* in 1838, and after Fizeau had studied the variation of the velocity of light in running water, according as the motions agree or differ in direction, Foucault was emboldened to attempt a measure of the *absolute* velocity of light, by an experiment which could be brought within the compass of a single room. I translate his own account of the arrangements made for this purpose.

"A pencil of solar light, reflected into a horizontal direction by a heliostat, falls upon the micrometric mark, which consists of a series of vertical lines distant from one another one-tenth of a millimetre. This mark, which in the experiment is the real standard of measure, has been divided very carefully by Froment. The rays, which have traversed this initial surface, fall upon a plane rotating mirror at the distance of a metre, where they suffer the first reflexion, which sends them to a concave mirror at the distance of four metres. Between these two mirrors, and as near as possible to the plane mirror, is placed an object-glass, having in one of its conjugate foci the virtual image of the mark, and in the other the surface of the concave mirror. These conditions being fulfilled, the pencil of light, after traversing the lens, forms an image of the mark on the surface of this concave mirror.

"Thence the pencil is reflected a second time, in a direction just oblique enough to avoid the rotating mirror, an image of which it forms in the air at a certain distance. At this place a second concave mirror is placed, facing so that the pencil, once more reflected, returns to the neighborhood of the first concave mirror, forming a second image of the mark. This is taken up by a third concave mirror, and so on to the formation of a last image of the mark on the surface of the last concave mirror of an odd number. I have been able to use five mirrors, which furnish a line twenty metres long for the ray to travel.

"The last of these mirrors, separated from the preceding one, which faces it, by a distance of four metres (equal to its radius of curvature), returns the pencil back on itself: a condition surely fulfilled when the returning image and the original image on the last mirror but one coalesce. Then we are sure that the pencil retraces its steps, returns in full to the plane mirror, and all the rays go back through the mark, point for point, as they went forth.

"This return of the pencil may be proved on an accessible image by reflecting the pencil to one side by a surface of glass at an angle of 45°, and examining it through a microscope of small power. The latter, resembling in all respects the micrometric microscopes in use for astronomical observations, forms, with the mark and the inclined glass, one solid piece of apparatus.

"The real image sent into the microscope, and formed by the returning rays partially reflected, occupies a definite position in relation to the glass and the mark itself. This position is precisely that of the virtual image of the mark seen by reflexion in the glass. At least, this is true when the plane rotating mirror is at rest. But when this mirror turns,

the image changes its place: for, while the light is going and returning between the mirrors, the plane mirror has shifted its position, and the returning rays do not strike at the same angle of incidence as when they left it. Hence the image is displaced in the direction of the rotation, and this displacement increases with the velocity of rotation: it also increases with the length of the route passed over by the rays, and with the distance of the mark from the plane mirror.

"If we call V the velocity of light, n the number of times the mirror turns in a second, l the distance between the plane mirror and the last concave mirror, r the distance of the mark from the turning mirror, and d the observed displacement, we have $V = \frac{8\pi n l r}{d}$: an expression which gives the velocity of light when the other quantities are separately measured. The distances l and r are measured directly by a rule. The deviation is observed micrometrically: it remains to show how the number of turns (n) of the mirror is found.

"Let us describe first how a constant velocity is imparted to the mirror. This mirror, of silvered glass, and fourteen millimetres in diameter, is mounted directly upon the axis of a small air-turbine, of a well known model, admirably constructed by Froment. The air is supplied by a high pressure bellows of Cavaillé-Colle, justly distinguished for the manufacture of great organs. As it is important that the pressure should be very constant, the air, after leaving the bellows, traverses a regulator, recently contrived by Cavaillé, in which the pressure does not vary by one-fifth of a millimetre in a column of water of thirty centimetres. The fluid flowing through the orifices of the turbine represents a motive power of remarkable constancy. On the other hand, the mirror, when accelerated, soon encounters in the surrounding air a resistance which for a given velocity is also perfectly constant. The moving body placed between these two forces, which tend to equilibrium, cannot fail to receive and to preserve a uniform velocity. Any check whatever, acting upon the flow of the water, allows this velocity to be regulated within very extensive limits."

"It remains, to estimate the number of turns, or rather to impress on the moving body a determined velocity. This problem has been completely resolved in the following manner. Between the microscope and the reflecting glass, a circular disc is placed, the edge of which, cut in fine teeth, encroaches upon the mark and partly intercepts it. The disc turns uniformly on itself, so that, if the image shines steadily, the teeth at its circumference escape detection from the rapidity of the motion. But the image is not permanent: it results from a series of discontinuous appearances equal in number to the revolutions of the mirror; and, whenever the teeth of the screen succeed one another with the same frequency, there is produced on the eye an illusion easily explained, which makes the teeth appear immovable. Suppose then that the disc, with n teeth in its circumference, turns once in a second, and that the turbine starts up. If, by regulating the flow of air, the teeth are made to appear fixed, we are certain that the mirror makes n turns in a second.

"Froment, who made the turbine, wished to invent and construct a chronometric wheelwork to move the disc. It is a remarkable piece of

clock-work, which resolves, in an elegant manner, the problem of uniform motion in the particular case in which there is no work to be done. The success is so complete that it is my daily experience to launch the mirror with 400 turns a second, and see the two pieces of apparatus march within $\frac{1}{10000}$ nearly of accordance during whole minutes.

"Notwithstanding the assurance I had gained in the measurement of time, I was surprised at proving in my results, discordances which were out of proportion to the precision of my means of measuring. After long research, I discovered the source of error in the micrometer, which did not allow of the degree of accuracy willingly attributed to it. To meet this difficulty, I have introduced into the system of observation a modification which amounts simply to a change of the variable. Instead of measuring micrometrically the deviation, I adopt for it a definite value in advance, suppose seven-tenths of a millimetre, or seven entire parts of the image; and I seek by experiment to find the distance between the mark and the turning mirror necessary to produce this deviation: the measures extending over a length of about a metre, the last fractions have a magnitude directly visible, and leave no room for error.

"By this means the apparatus has been purged of the principal cause of uncertainty: henceforth the results accorded, within the limits of errors of observation, and the means are settled in such a way that I am able to assign confidently the new number which appears to me to express nearly the velocity of light in space, viz: 298,000 kilometres in a second of mean time."

This value, reduced to statute miles, shows that the velocity of light is 185,177 miles in a second; which is less by 6336 miles than the velocity for light usually admitted into science, viz: the velocity obtained from the aberration of light. This discrepancy between the results of experiment and that of the astronomical determination, which comes nearest to it, is three times greater than the variation between the velocity deduced from aberration and that derived from eclipses.

Foucault states that the extreme difference of the results of various trials amounted to only $\frac{1}{100}$ of the whole quantity, and that the mean result can be trusted to the fraction of $\frac{1}{200}$. Moreover, the aberration of 20″·45, adopted by astronomers, cannot be supposed at fault by more than $\frac{1}{1000}$ of the whole. Neither the velocity by Foucault's experiment nor the value of aberration can be charged with a possible error of three per cent, or of any error approaching to this large discrepancy. How is the new velocity of light to be reconciled with the old value of aberration? I have said that aberration establishes only the *ratio* between the velocity of light and the velocity of the earth. If this ratio cannot be tampered with, and if one term of it (the velocity of light) must be diminished by three per cent, to suit Foucault's experiment, then we must at the same time diminish the other term (the velocity of the earth) proportionally; and the old ratio will be preserved, and the

value of aberration will be left unchanged. Is it possible, therefore, that there can be an uncertainty to the extent of three per cent in the velocity of the earth? If so, the tables are turned: and, instead of employing the ratio which aberration supplies to calculate the velocity of light from the velocity of the earth, as the best known of the two, we henceforth must calculate the velocity of the earth from the velocity of light. For, Foucault has found the latter by experiment more accurately than astronomy gives the former. If there is an error of three per cent in the velocity of the earth, it is an error in space and not in time. To diminish the velocity of the earth sufficiently by a change of time would demand an increase in the length of the year amounting to eleven days nearly.

The only other way of reaching the velocity of the earth is by diminishing the circumference of the earth's orbit, and this, if diminished, changes proportionally the mean radius of the orbit; that is, the sun's mean distance. The question, therefore, resolves itself into this. Can the distance of the sun from the earth be considered uncertain to the extent of three per cent of the whole distance?

The answer to this question will lead me into a brief discussion of the processes by which the sun's distance from the earth has been determined, and the limits of accuracy which belong to the received value. To see the distance of any body is an act of *binocular* vision. When the body is near, the two eyes of the same individual converge upon it. The interval between the eyes is the little base-line, and the angle which the optic axes of the two eyes, when directed to the body, make with each other is the parallax; and by this simple triangulation, in which an instinctive geometrical sense supersedes the use of sines and logarithms, the distance of an object is roughly calculated. As the distance of the object increases, the base-line must be enlarged; but the geometrical method is the same, even when the object is a star and the base of the triangle the diameter of the earth's orbit. Substitute then for the two eyes of the same observer the two telescopes of different astronomers, planted at the opposite extremities of the earth's diameter, and any one will understand the principle upon which the binocular eye of science takes its stereoscopic view of the universe, and plunges into the depths of space. In this way it is that the distance of the sun from the earth is associated with the *solar parallax:* which is the angle between the directions in which two astronomers point their telescopes when they are looking at the sun at the same moment. To know the sun's distance, the astronomer studies the solar parallax. As Kepler's third law establishes a relation between the distances of the different planets from the sun, and their periods of revolution, if the astronomer finds either dis-

tance by observation, the others can be computed from this law. As the solar parallax is only about eight seconds, and an error of one-tenth of a second includes an error of more than a million of miles in the sun's distance, he takes advantage of the law of Kepler, and selects a planet which comes occasionally nearer to the earth than the sun. The choice lies between Venus at inferior conjunction and Mars at opposition. The parallax of Mars may vary from 20″·7 to 19″·1, according to the positions of Mars and the earth with respect to the perihelion of the orbit at the time of opposition. The parallax of Venus at conjunction may vary, for the same reasons, from 33″·9 to 29″·9. Venus, therefore, may be nearer to the earth than Mars, and the parallax more favorable. But Venus cannot be seen at conjunction except when its latitude is so small that a transit across the sun's disc occurs. Then the two observers refer its place not to a star but to the sun, and the quantity they determine is the difference of parallax between Venus and the sun; which will vary from about 21″ to 25″. Moreover, the difference of parallax is measured, not directly, but through the influence it produces on the duration of the transit at the two stations: and, therefore, upon a greatly enlarged scale.

What are the results which have been obtained: 1st, by observations of the transits of Venus, and 2d, by observations on Mars at opposition?

1. Only two transits of Venus have occurred since the time when the sagacious Dr. Halley invoked the attention of posterity to these rare astronomical events as pregnant with the grandest results to science; viz: those of 1761 and 1769. The astronomers of the last century did not neglect the charge which Halley consigned to them. The transit of 1769 was eminently favorable, offering a chance which comes only once in a millenium, as Professor Winthrop happily explained in his lectures on the last transits.

Whatever verdict posterity shall pronounce on the deductions from the observations then made, they will never, says Encke, reproach astronomers or governments with negligence or want of appreciation towards this golden opportunity. The solar parallax which Encke deduced from an elaborate discussion of all the observations, fifty years after they were made, is 8″·57116. This corresponds to a solar distance of 95,360,000 statute miles.

Although transits of Venus will take place in 1874 and 1882, and astronomers already begin to talk of preparing for them, I have the authority of Encke for declaring that, in comparison with that of 1769, the next two transits will be so unfavorable that nothing short of perfection in the construction of instruments, and in the art of observing, can compensate for the natu-

ral disadvantage: so that the reduction of the possible error in the sun's parallax within the limit of one one-hundredth of a second is hopeless for at least two centuries more.

2. The solar parallax may also be derived from the parallax of Mars, when this planet is in opposition. In 1740 the French astronomer, Lacaille, was sent to the Cape of Good Hope, and from the parallactic angle observed between the direction of Mars as seen from that station and from the observatory of Paris (deduced from observations of declination), the horizontal parallax of Mars was computed, and consequently that of the sun. The solar parallax thus found was 10″·20, with a possible error not exceeding 0″·25. Henderson, by comparing his own observations of the declination of Mars at its opposition in 1832 with corresponding observations at Greenwich, Cambridge, and Altona, computed the solar parallax at 9″·028.

The United States Naval Astronomical expedition to Chili, under the charge of Lieut. J. M. Gilliss, during the years 1849–1852, had for its object the advancement of our knowledge of the solar parallax, partly by observations of Mars at opposition, and partly by observations of Venus during the retrograde portion of her orbit, and especially at the stationary points, in conformity with a method suggested by Dr. Gerling; the whole to be compared with simultaneous observations at northern observatories. Although the observations at Chili were made on 217 nights, covering a period of nearly three years, the coöperation of northern astronomers was so insufficient that only 28 corresponding observations were made. On this account the second conjunction of Venus was useless: the other conjunction of Venus and the second opposition of Mars were of little value; and even the first opposition of Mars led to no significant result. Dr. B. A. Gould has computed the solar parallax from the first opposition of Mars, observed at Chili, at 8″·50.

3. The solar parallax can also be computed from the law of universal gravitation. The principle may be thus stated. The motion of the moon round the earth is disturbed by the unequal attraction of the sun on the two bodies. The magnitude of the disturbance will be in some proportion to the distance of the disturber when compared with the relative distance of the two disturbed bodies: and this ratio of distances is the inverse ratio of the parallaxes of the sun and moon. By selecting one of the perturbations in the moon's longitude particularly adapted to this purpose, Mayer, as early as 1760, computed the solar parallax at 7″·8. In 1824, Burg calculated this parallax from better observations at 8″·62. Laplace gives it at 8″·61. Fontenelle had said that Newton, without getting out of his arm-chair, found the figure of the earth more accurately than others had done by going to the ends of the earth. Laplace makes a

similar reflexion on this new triumph of theory. "It is wonderful that an astronomer, without going out of his observatory, should be able to determine exactly the size and figure of the earth, and its distance from the sun and moon, simply by comparing his observations with analysis, the knowledge of which formerly demanded long and laborious voyages into both hemispheres." The accordance of the results obtained by the two methods is one of the most striking proofs of universal gravitation. Pontecoulant makes the solar parallax by this method 8″·63. Lubboch, by combining Airy's empirical determination of the coëfficient with the mass of the moon as he finds it from the tides (viz: $\frac{1}{6}\frac{1}{7}$), makes the solar parallax 8″·84. If the mass of $\frac{1}{7}\frac{1}{5}$ is substituted, the parallax is changed to 8″·81. Finally, Hansen, in his new *Tables of the Moon*, adopts 8″·8762 as the value of the solar parallax. Moreover, Leverrier, in his *Theory of the apparent motion of the Sun*, deduces a solar parallax of 8″·95 from the phenomena of precession and nutation.

The conclusions of this whole review are summed up in the following table: in which the values of the solar parallax and of the sun's distance, by the three methods of astronomy, and by the experiment of Foucault, are placed in juxtaposition: also the different velocities of light found by astronomical observations and by experiment.

Observer or Computer.	Method.	Parallax.	Distance.
Encke,	By Venus (1761),	8″·53	95141830 miles.
Encke,	" " (1769),	8 ·59	95820610
Lacaille,	By Mars,	10″·20	76927900
Henderson,	" "	9 ·03	90164110
Gilliss and Gould,	" "	8 ·50	96160000
Mayer,	By Moon,	7″·80	104079100
Burg,	" "	8 ·62	94802440
Laplace,	" "	8 ·61	94915970
Pontecoulant,	" "	8 ·63	94689710
Lubboch,	" "	8 ·84	92313580
"	" "	8 ·81	92652970
Hansen,	" "	8 ·88	91861060
Leverrier,	" "	8 ·95	91066350
Foucault,	By light,	8″·86	92087342
Fizeau,	" "	8 ·51	95117000
Velocity of light,	By eclipses,		193350
" "	" aberration,		191513
" "	" Fizeau's experiment,		194667
" "	" Foucault's experiment,		185177

Foucault's experiment on the velocity of light has been popularly announced as making a "revolution in astronomical science." But it appears from the preceding sketch that it has raised no new question in astronomy, though it may have at-

tracted popular attention to an old difficulty, and possibly given a solution to it. The three astronomical methods present solar distances, which, even if we select the most trustworthy decision of each, differ by three or four millions of miles: that is, by three or four per cent of the whole quantity. Though the best products of the first and third methods were at one time within a million of miles of each other, an increase of lunar observations, and especially improvements in the lunar tables, have now carried that difference up to four millions of miles. If Foucault's experiment were allowed to give the casting vote, it would decide in favor of the third method; thus making the reflexion of Laplace, which I have already quoted, still more memorable.

In regard to the commonly received distance of the sun, which is based upon Encke's profound discussion of all the observations made at the last two transits of Venus, the case stands thus. Encke decides, from the weights of the observations, discussed in the light of the mathematical principle of *least squares*, that the probable error of the sun's distance, as given by the transits, does not exceed $\frac{1}{2}\frac{1}{3}\frac{1}{9}$ of the whole quantity. Astronomers have also reason to believe that the adopted value of aberration is correct within $\frac{1}{1}\frac{1}{8}\frac{1}{0}\frac{1}{8}$ of the whole quantity. Moreover, Foucault is confident of his determination of the velocity of light within $\frac{1}{6}\frac{1}{0}\frac{1}{0}$ of the whole quantity; nay, he expects to improve his instruments so as to banish all errors larger than $\frac{1}{6}\frac{1}{0}\frac{1}{0}\frac{1}{0}$ of the whole quantity. Neither the velocity of light, aberration, nor the sun's distance can be suspected of an error to the extent of three or four per cent; and yet one at least must be wrong to this degree, as the best values of the three elements are irreconcilable with each other. Which shall be changed?

It may excite surprise in those who have heard of the *accuracy* of astronomy, without weighing the exact significance of the word as applied to so large a subject, that there should still be a lingering uncertainty, to the extent of three or four millions of miles, in the sun's distance from the earth. But the error, whatever it is, is propagated from the solar system into the deepest spaces which the telescope has ever traversed. The sun's distance is the measuring rod with which the astronomer metes out the distances of the fixed stars and the dimensions of stellar orbits. An error of three per cent in the sun's distance entails an error of three per cent in all these other distances and dimensions. Trifling as three per cent may seem, the correction runs up to 600,000 millions of miles in the distance of the nearest fixed star!

ART. XVI.—*Further Remarks on a method of Reducing Observations of Temperature;* by Professor J. D. EVERETT, of Kings College, Windsor, Nova Scotia.

IN an article in the January number of this Journal, I recommended for general use a method of comparing climates, as regards temperature, by reference to the curve whose equation is

$$y=A_0+A_1 \sin (x+E_1)$$

Professor Loomis, in his "*Remarks*" appended to my article, having impugned the accuracy of this mode of procedure, on the ground that the curve of temperature at a place does not possess the symmetry which characterizes the curve expressed by the above equation, I propose, in the present paper, to furnish a thorough investigation of the degree of accuracy which is attained.

We must first prove the following proposition: Any m numbers, (m being either $2n$ or $2n+1$,) can be *exactly* expressed by an equation of the form

$$y=A_0+A_1 \sin (x+E_1)+A_2 \sin (2x+E_2)+ \ldots . +A_n \sin (nx+E_n),$$

in such a sense that, by giving x the successive values,

$$0, \frac{1}{m}\times 360°, \frac{2}{m}\times 360° \ldots . \frac{m-1}{m}\times 360°,$$

the resulting values of y will be the given numbers in order. We shall hereafter denote the terms on the second side of the above equation, by the symbols t_0, t_1, t_2, &c. To prove the proposition, let the series be transformed, (see p. 25 of my former article,) into

$$y=A_0+P_1 \cos x+Q_1 \sin x+P_2 \cos 2x+Q_2 \sin 2x+ \ldots . +P_n \cos nx+Q_n \sin nx.$$

We can then form m equations, by writing the given numbers in order for y, and giving x the corresponding values above indicated. If $m=2n+1$, we shall have $2n+1$ equations, to determine the $2n+1$ constants A_0, P_1, Q_1, &c. If $m=2n$, the last term, $Q_n \sin nx$, is to be omitted; and we shall have $2n$ equations to determine $2n$ constants. Hence, by the theory of equations, there will always be one and only one solution.

The rule for obtaining the solution is extremely simple:—

To find the value of any one of the constants, multiply each equation by its coefficient of that constant, and add the m equations thus obtained.[1] It will be found that all the terms on the

[1] It is worthy of remark that this rule is identical with that laid down, in the theory of probabilities, for determining the most probable values of any number of unknown quantities from a greater number of simple equations, when the latter are all of equal weight. This general rule is thus given in "*Airy on Errors of Observations*," p. 80: "Multiply every equation by its coefficient of one unknown quantity, and take the sum for a new equation; the same for the second unknown quantity; and so on for every unknown quantity; and thus a number of equations will be found equal to the number of unknown quantities."

second side destroy one another, except those which contain the constant we are seeking, and the sum of these will be $\frac{m}{2}$ times the constant, except when the constant is P_n in the case where $m=2n$, for which the sum will be mP_n. When there are 12 given numbers, this process resolves itself into that described in my former article,[2] (*this Journal*, [2], xxxv, 17), supplemented by the following formulæ:

$$6P_5=k_0-s_2(k_1-k_5)+s_1(k_2-k_4)$$
$$6Q_5=\quad s_1(k_1-k_5)-s_2(k_2-k_4)+k_3$$
$$12P_6=K_0+K_2+K_4-K_1-K_3-K_5$$

The monthly means at Greenwich, derived from the table in Professor Loomis' "*Remarks*," reckoning the 31st of January and 1st of March as part of February, are

36·6 38·6 41·4 46·2 52·9 59·0 61·8 61·0 56·5 49·9 43·2 39·3

These are exactly expressed by the formula,

$$y=48.87+12.44\sin(x+262°\ 31')+\cdot84\sin(2x+57°\ 44')+\cdot18\sin(3x+338°\ 12')$$
$$+\cdot25\sin(4x+253°\ 26')+\cdot20\sin(5x+252°\ 29')+\cdot13\sin(6x+270°),$$

where x is 0° for January, 30° for February, 60° for March, and so on.

Here t_0 is the constant 48·87.

The values of t_1, for the 12 months in order, are

−12·33 −11·49 −7·57 −1·62 +4·76 +9·87 +12·33 +11·49 +7·57
+1·62 −4·76 −9·87.

The values of t_2 are

+·71 +·74 +·03 −·71 −·74 −·03 +·71 +74 +·03 −·71 −74 −·03

These values, it will be observed, repeat themselves after the first six; and the sum of any consecutive six is 0.

The values of t_3 are

−·07 +·17 +·07 −·17 −·07 +·17 +·07 − ·17 −·07 +·17 +·07 −·17

which repeat themselves after the first four, or go through their cycle in a third of a year. Also the sum of any consecutive four is 0.

The values of t_4 are

−·24 +·06 +·18 −·24 +·06 +·18 −·24 +·06 +·18 −·24 +·06 +·18

which go through their cycle 4 times in the year. Also the sum of any consecutive three is 0.

The values of t_5 are

−·19 +·13 −·04 −·06 +·15 −·19 +·19 −·13 +·04 +·06 −·15 +·19

[2] The following errata in the formulæ there given require correction:—
p. 26, lines 9, 10, 11, for K_0+K_3 read K_0-K_3
" K_1+K_4 " K_1-K_4
" K_2+K_5 " K_2-K_5

which do not so plainly show the recurrence of cycles, because 5 is not a measure of 12. It is easy however to trace 5 maxima and 5 minima.

The values of t_0 are

$-\cdot13\ +\cdot13\ -\cdot13\ +\cdot13\ -\cdot13\ +\cdot13\ -\cdot13\ +\cdot13\ -\cdot13\ +\cdot13\ -13\ +\cdot13$

which go through their cycle 6 times in the year. Also the sum of any consecutive two is 0.

By addition, we find the values of $t_0+t_1+t_2+t_3+t_4+t_5+t_6$ to be

36·62 38·61 41·41 46·20 52·90 59·00 61·80 60·99 56·49 49·90 43·22 39·30

or, to one place of decimals,

36·6 38·6 41·4 46·2 52·9 59·0 61·8 61·0 56·5 49·9 43·2 39·3

which agree exactly with the monthly means.

The error committed by stopping at any term, is, of course, equal to the sum of the terms omitted. These sums are given in the following table; the first line containing the sum of all the terms after t_1, the second line the sum of all after t_2, and so on,—

+·08	+1·23	+·11	−1·05	−·73	+·26	+·60	+·63	+·05	−·59	−·87	+·30
−·63	+·49	+·08	−·34	+·01	+·29	−·11	−·11	+·02	+·12	−·13	+·33
−·56	+·32	+·01	−·17	+·08	+·12	−·18	+·06	+·09	−·05	−·20	+·50
−·32	+·26	−·17	+·07	+·02	−·06	+·06	+·00	−·09	+·19	−·26	+·32
−·13	+·13	−·13	+·13	−·13	+·13	−·13	+·13	−·13	+·13	−·13	+·13

The terms t_2, t_4, t_6 produce no effect upon the mean temperature of a half-year, for the sum of any 6 consecutive values of these terms is 0; hence, in deriving the mean temperature of a half-year from t_0+t_1, the error committed depends only on t_3 and t_5. The sums of t_3 and t_5, for each month, are

$-\cdot26\ +\cdot30\ +\cdot03\ -\cdot23\ +\cdot08\ -\cdot02\ +\cdot26\ -\cdot30\ -\cdot03\ +\cdot23\ -\cdot08\ +\cdot02$

and the means of every consecutive six of these, commencing with October—March, are

$+\cdot04\ -\cdot04\ -\cdot01\ -\cdot02\ +\cdot07\ -\cdot03\ -\cdot04\ +\cdot04\ +\cdot01\ +\cdot02\ -\cdot07\ +\cdot03$

which are the corrections required in determining the mean temperature of 6 consecutive calendar months from t_0+t_1. In fact, the means of t_0+t_1 for every 6 months, in the above order, are

41·47 40·93 42·52 45·81 49·92 53·75 56·27 56·81 55·22 51·93 47·82 43·99

while the means of the actual monthly temperatures are

41·50 40·88 42·50 45·78 49·98 53·72 56·23 56·85 55·23 51·95 47·75 44·02

showing the same corrections as above, allowing differences of 1 in the last figure for neglected places of decimals. Hence we see that the mean temperature of any 6 consecutive months at Greenwich can be calculated from t_0 and t_1 (or from their constants A_0 A_1 E_1), subject to errors not exceeding ·07 of a degree.

It may be shown in general, by a theorem proved on p. 30 of my former article, that, if the expression for monthly means be

$$y=t_0+t_1+t_2+t_3+t_4+t_5+t_6,$$

the expression for half-yearly means will be

$$Y=t_0+\cdot644t_1-\cdot236t_3+\cdot173t_5;$$

that is,

$$Y=A_0+\cdot644\,A_1\sin(x+E_1)-\cdot236A_3\sin(3x+E_3)+\cdot173A_5\sin(5x+E_5)$$

and since $\cdot236\,A_3$ and $\cdot173\,A_5$ are practically insignificant compared with $\cdot644\,A_1$, the two last terms may be neglected.

For Greenwich, we have

$$\begin{aligned}\cdot644A_1&=\cdot644\times12\cdot44=8\cdot01,\\ \cdot236A_3&=\cdot236\times\ \cdot18=\ \cdot04,\\ \cdot173A_5&=\cdot173\times\ \cdot20=\ \cdot03.\end{aligned}$$

Hence the sum of the two last terms in the expression for Y cannot exceed $\cdot04+\cdot03=\cdot07$, which is only $\frac{1}{11\frac{1}{2}}$ of $\cdot644\,A_1$; and the greatest error produced by the omission of these terms in comparing two halves of the year cannot be more than about $\frac{1}{11\frac{1}{2}}$ of the difference between the greatest and least values of Y. It is to be observed that the half-yearly means denoted by Y are not limited to means of 6 calendar months, but may belong to any $182\frac{1}{2}$ consecutive days.

It thus appears that the values of A_0, A_1, E_1 are sufficient to determine, with competent accuracy, the mean temperature of any half-year, and hence to determine the range as measured by the difference between the warmest and coldest halves of the year, a system of measurement which I think is as fair as any that can be devised.

The determination of the precise dates of maximum and minimum is always difficult, whatever method be pursued, owing to the slow rate at which temperature changes when near its maximum or minimum. Even the table of daily temperatures at Greenwich, though derived from an average of 43 years, leaves both these dates doubtful, as a glance will show. It is always much easier to determine the dates at which the temperature is equal to the mean of the year, (or at which the curve of temperature intersects the line of mean annual temperature,) because at and near these dates the temperature changes with its greatest rapidity. This remark applies to half-yearly means, as well as to daily temperatures; hence we can determine more precisely "the dates which divide the year into halves whose temperatures are each equal to the mean of the year," than the dates which are the centres respectively of the warmest and coldest halves of the year. For the former determination depends upon the dates at which the value of Y is equal to the mean annual temperature, while the latter depends upon the dates at which Y is a maximum or minimum. And the above phrase in inverted commas may be advantageously substituted for "centres of warm

and cold halves of the year" in the definitions of E_1 in my former article.

We will now proceed to test our determinations of range and date by the Greenwich table.

The range, as measured by the difference between the warmest and coldest halves of the year, is by our theory equal to $A_1 \times 1{\cdot}2879 = 12{\cdot}44 \times 1{\cdot}2879 = 16{\cdot}05$. This is in error by about ·07, the warmest 182 days, April 22—Oct. 30, having a mean temperature of 57·00, and the coldest 183 days, Oct. 31—April 21, a mean temperature of 40·88, showing a difference of 16·12.

As regards date, the value of E_1 is 262° 31′ or 82° 31′, according as we make A_1 positive or negative. Subtracting 15° from the latter, to reduce to the beginning of the year, we have for remainder 67° 31′, the complement of which is 22° 29′, equivalent to 22·8 days; and half a year or 182·5 days, added to this, gives 205·3 days. The 22d and 205th days of the year are January 22 and July 24; hence, by our theory, January 22·8 and July 24·3 should divide the year into halves whose mean temperatures are equal to each other and to the mean annual temperature.

From the Greenwich table we derive the following half-yearly means:—

Jan. 23	July 23, both inclusive,	182 days, mean temp.	49·0
23	24	183	49·2
22	22	182	48·8
22	23	183	48·9

the mean annual temperature being 48·9. Hence our determination of date is only 1 day in error.

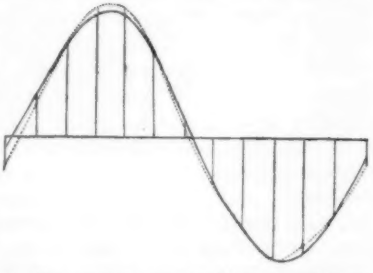
The centres of the warmest and coldest halves, as already stated, cannot be determined with so much precision. In the present case, the coldest 183 days are October 21—April 21, the centre of which is January 20, whereas our theory makes it January 22·8.

We have a good approximation, in the present case, to the dates which are midway between those whose temperatures are the same as the mean of the year. The latter, (which we may call the Vernal and Autumnal intersections,) are April 29 and October 21; and the dates midway between these are January 23·5 and July 25, which agree closely with January 22·8 and July 24·3. I believe this agreement will be generally found to exist, because the term t_1 is changing its value with nearly maximum rapidity at the dates of intersection, and thus overpowers the other terms, especially as t_2, the most considerable of them, cannot hasten or retard one of the intersections to any considerable extent, without producing an opposite and nearly equal effect upon the other.

The influence of the term t_2, in modifying the symmetrical curve $y=t_0+t_1$ for Greenwich, may be thus described:—It retards the vernal intersection, and the maximum; hastens the autumnal intersection, and the minimum; intensifies the maximum, and moderates the minimum. Hence the temperature rises higher above the mean than it falls below; but the number of days below the mean is greater than the number above. These features of climate will probably be found to prevail generally in extra-tropical latitudes; and they constitute the most marked departures of the curve of temperature from symmetry, the terms after t_2 being comparatively insignificant. In so far as they are common to all places, they are unimportant in the comparison of climates, which was the object proposed in my former paper; and, at all events, the first elements that claim attention are those in which the actual curve agrees with the symmetrical curve, that is to say, the elements of mean temperature, range, and general earliness, as represented by the three constants A_0 A_1 and E_1.

Of course, three numbers cannot express every feature of the curve of temperature at a place, for it is in the nature of things impossible that three numbers should express more than three independent elements. If the constants A_0 A_1 and E_1 are correct as measures of the three leading elements, it is all that we can expect of them; and if a closer approximation to the curve of temperature is desired, it can readily be obtained by computing the constants in one or more of the succeeding terms.

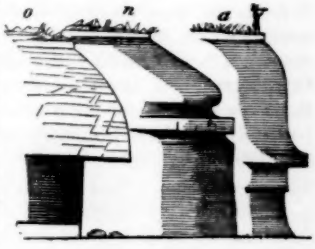
The subjoined diagram will give a clearer impression of the relation between the symmetrical and the actual curve than language can convey. The continuous line is the symmetrical curve for Greenwich; and the dotted line is a curve, drawn through points whose ordinates are the mean temperatures of the 12 months, beginning and ending with April, these points being situated on the vertical lines in the diagram. The horizontal line represents the mean annual temperature. Of the areas intercepted between the two curves, those which lie above either of the curves are together equal to those which lie below the same.



ART. XVII.—*On the Coal Measures of Cape Breton, N. B., with a Section;* by J. P. LESLEY.[1] (Communicated by the author.)

THE following section was obtained in August, 1862, from the cliffs between Lingan and Great Glace Bays, on the east coast of Cape Breton, from sixteen to twenty miles east of Sydney. Part of it was made out by means of a rope and ladder let down from the upper edge of the cliffs, where these overhung the sea, or occupied intervals between the short sand and gravel beaches. At the upper limit, which is also the northwestern end of the section, a square headland projects into the Gulf of St. Lawrence, along the axis of a synclinal basin with sloping sides of 4° or 5° dip. From this headland southeastward, the section was made out by an examination of each layer as it emerged from the sea, past the mouth of Little Glace Bay (where the new harbor is constructing, under the skillful and energetic direction of Captain William P. Parrot, Civil Engineer, of Boston, Mass.) and as far as to the mouth of Great Glace Bay.

	Feet.
Soft shales with a hard belt at the bottom, - - - - -	20



These rocks cap the square headland projecting into the Gulf of St. Lawrence between the Burnt Head and Little Glace Bay. They are the highest Coal-measure rocks of this basin, and perhaps the highest Coal-measures south of Sydney Bay. The cliffs are about forty feet high, and exhibit a remarkable contour, caricaturing the human face in profile, by means of the overhanging ledge of hard sandrock at the bottom of the mass, and about half-way of the height of the cliff. See woodcut (*a*).

Red shale belt: red and green 10; red 10; green 2; red 1½; green ½; red 1½; green 1½, - - - - - - - - - -	27
Fire clay, the upper 5 feet crowded with small nodules of carbonate of iron; middle 2 feet sandy; lower 5 pure, - - - - -	12
Red shale 2 feet, over 2 feet of fire-clay, under which runs the outcrop of a plate of carbonate of iron, from 4 to 8 inches thick, for hundreds of yards along the face of the cliff, - - - -	4
Shales, with three black streaks, like the outcrops of coal beds, but mere discolorations of the shale: layers of small nodules of iron occur in the lower 10 feet; the lowest 2 feet are fire-clay, -	22
Sandstone cliffs 8 feet, over sandy shales 6 feet, - - - -	14
Coal; good; on fire-clay passing down into, - - - - -	1
Sandstone 6 feet; genuine black slate 2; fire-clay 8, - - -	16

[1] Reprinted (with many changes and additions by the author) from the *Proceedings of the American Philosophical Society.*

Slate cliffs; the top rock of the great coal bed; varying in compactness, but essentially a homogeneous mass of finely levigated and foliated sandy mud, - - - - - - - - - - 40

Coal; the Hub Vein; slate 1½, soft coal 1½, solid 4, hard 1, - 8

Of this, only six feet is good workable coal, on the coast; but it increases westward, and with the omission of eighteen inches poorer top coal, yields from six to seven feet of good body coal. It is on this bed that the principal mines of the Glace Bay Company are situated, shipments, until lately, having been made from a long pier projecting into the sea, or in an open roadstead by lighters. Now, a railroad a mile long, on a sixty foot summit, lands the coal upon a long wharf occupying the north side of an artificial harbor, constructed out of the shallow Little Glace Bay. The old drift-works into the bed have been abandoned on account of the inflowing of the tide, and new works by a slope have been commenced a quarter of a mile inland.—The floor of the bed is not well seen, being covered by the ruins occasioned by the firing of the cliff coal, in the last century, by the French, after the fall of Louisburg, and the cession of Acadia to the British government. The miners report the sandstone, next to be described, as lying immediately beneath the coal.

Sandrock full of the moulds of plants, mostly stems, only occasionally blackened, - - - - - - - - - - - 20

This mass of building stone is a rare exhibition for these Coal-measures. It forms the long point on which the pier is built. Its thickness could not be exactly determined, because, like all the very sandy deposits of the section, it is false-bedded and variable. It is as true here as in the great Coal-measures of the United States that the principal masses of sandstone are reserved for the lowest parts of the formation. The great sandrocks of Cape Breton underlie all the productive Coal-measures, and are seen around Sydney.

Cannel coal bed. This is no true cannel but a coal shale, compactly foliated, highly bituminous, burning well, but with much ash, and crowded with fish-scales and minute shells. It varies, and sometimes reads thus; cannel, 8 inches; bituminous coal, 8 inches; clay, 1½ inch; bituminous coal, 3 inches, - - - - - 1½

Fire-clay, - - - - - - - - - - - 6

Cannel coal, as above, - - - - - - - - - - ½

Fire-clay, - - - - - - - - - - - 2½

Sandstone cliff rocks 8 feet, over sandy shales 11 feet, - - 25

Cannel coal, or jet black slate; sometimes growing compact like cannel, but nowhere seen as a true coal, but rather a black fire-clay, one inch thick, with a few inches of black slate above and below it; plenty of fish scales, but no ferns, - - - - ½

Fire-clay 3; sandy clay 3, shaly 3, pure clay 4, blackish shales 8½, soft clay 2,—clay full of balls passing an eight inch plate of iron ore,—sandy shales 6, soft 3½, dark soft 1½, soft gray 8, - - 45

Sandstone shales 2, gray shales 5, blackish 1, gray 6, massive weathering flaky 2, sandy flaky 9, sandy cliff shales 11, blackish 10, sandy cliff shales 5, sandy shales 20, clay descending into sandstone 21. In this last occurs half an inch of coal, - - 92

Top slate, - - - - - - - - - - - - 6
Coal; sometimes black slate with two inches of coal, - - $\frac{1}{2}$
Fire-clay, - - - - - - - - - - - - 5
Shales: blackish soft 4, gray 2, with poor sandy ball ore $\frac{1}{2}$, gray 4, sandstone flinty 1, fire-clay, compact below, 6, sandy shales 6, yellow 6, gray 6, soft sandy gray 12, soft shales (nipped out) 5, false-bedded sandy shales, hard at top, soft at bottom, 17, - - $69\frac{1}{2}$

This great mass of sandstone, thrown up at a steep angle, not by any general structural movement, but by original oblique deposition, has here resisted the wearing action of the waves, and left a curious and instructive promontory. The mass begins at the bottom with 3 inches of pure clay, under which is an inch of

Cannel coal which burns well and is full of fish scales.

Shales, soft yellow, concretionary, clay slates 7, harder 1, gray $2\frac{1}{2}$, with iron nodules along its base, gray 4, soft blackish band $\frac{1}{4}$, sandy foliated 3, top clay 1, gray, blackish $\frac{1}{2}$ foot, - - - $19\frac{1}{2}$
Cannel coal, or flaming slate $\frac{1}{2}$, - - - - - - - 1
Hard shale $\frac{3}{8}$, coaly matter half an inch, hard sandy shales 3, compact fire-clay 8, - - - - - - - - - - 11

These are the lowest rocks seen before reaching Little Glace Bay entrance, in the low banks, which fall off suddenly into the deep channel of the bay. A slight break in the section takes place here; it cannot be more than a few feet. The section commences again at the summit of the headland projecting from the south side of the bay, and runs thence uninterruptedly to the mouth of Great Glace Bay.

Soft measures under the soil, - - - - - - - - 10
Coaly top slate 4 inches, bituminous coal 4 inches, - - - $\frac{1}{2}$
Sandrock variable 1, green clay with horses of sand $\frac{1}{2}$, fire-clay 2, more compact 2, more sandy, becoming sandstone, 2, compact sandrock with thin flag-courses 7, - - - - - - $14\frac{1}{2}$
Shale fire clay 3, in pencils 4, sandy compact $3\frac{1}{2}$, in pencils 6, sandy 15, crumbling 4, - - - - - - - - - $35\frac{1}{2}$

The profile of this mass is one of singular architectural beauty. See woodcut (p. 179). (*n*).

Sandrock 8, blackish shales and fire-clays 4, sandrock massive 10, sandy fire-clay 2, shaly sandstone with six inch courses $7\frac{1}{2}$, dark shales $7\frac{1}{2}$, flags 3, gray top shales $1\frac{1}{2}$, - - - - - - $43\frac{1}{2}$
Bituminous slates with one inch of cannel in the midst, - - 1
Shale fire-clay 1, sandy 1, sandstone 2, sandy 11, fire-clay 5; the whole forming cliffs beetling over the breakers (woodcut *o*), - $20\frac{1}{2}$
Coal. Harbor vein, - - - - - - - - - - 5

Wrought by the inhabitants for many years in an entry from the beach. A new opening has been made on the outcrop where it crosses to the northwest side of the harbor below the new bridge.

Shales foliated, under which lies a plate of carbonate of iron three inches thick, sometimes breaking up into balls, - - - $8\frac{1}{4}$
Coals, with a centre streak of jet, perhaps characteristic of the bed, for it appears again in it at the new bridge, - - - - $\frac{2}{3}$
Shales, red, green, yellow $7\frac{1}{2}$, hard clay sandstone 2, clay shale 5, $14\frac{1}{2}$

Coal. Regular bed of bituminous coal, - - - - - - 2

Sandy shales, foliated; then compact; then in half inch layers 26, sandstone then sandy shales 10, - - - - - - - - 36

Shales, gray, blackish outside, 5, shaly fire-clay 10, - - - 15

Sandstone, greenish, 6, contorted 8; the local false-bedding has formed the point, and, like one or two other exposures along this coast, would throw a geologist completely off the track, leading him to suppose the country infested with high dips and faults; whereas, careful instrumentation has demonstrated an extraordinarily quiet and regular condition of things, - - - - - 14

Fire-clay 2, shales gray, green, harsh 4, gray, green 5, soft gray 20, 31

Sandrock in three equal layers, - - - - - - - - 6

Soft fire-clay: top slate with nodules of ore, - - - - - 10

Coal half an inch, black slate six inches, - - - - - - $\frac{1}{2}$

Fire-clay, passing further on into red, green and yellow shales; then sandy 6, false-bedded shales 12, ferruginous fire-clay 2, hard blackish slates 8, - - - - - - - - - - 28

Sandstone, green, rough, shaly, passing into dark shales 12, beautifully false-bedded, scalloped in all directions like the blocks and faces of No. X (Upper Devonian) at the viaduct of the Conemaugh, in Cambria County, Pennsylvania;—thin massive sandstone 6 feet, - - - - - - - - - - - 18

Shales, yellow sandstone at top, becoming yellow shales and then at bottom black, - - - - - - - - - - - 20

Carbonate of lime and iron, a tight blue bed, - - - - - 5

Sometimes $1\frac{1}{2}$ feet thick, but will not average more than 10 or 11 inches. It forms a long reef into the sea, in the exact line of the distant headland. As a solitary specimen of this kind of rock in this section, it is all the more important to have it carefully traced inland. It rests on a green fire-clay full of nodules of ore, as large as filberts and walnuts, oxydized on the surface.

Blackish top slate, under which is a carbonaceous streak, - - 3

Shales (at the top sandstone balls a foot thick), yellow, then green and full of nodules of ore 11, soft fire-clay 1, yellow, then sandy, then clayey, then fire-clay 8, blackish fire-clay, then gray 10, - 30

Coal, - - - - - - - - - - - - - - 2

Fire-clay 2, with nodules of ore 2, blue shales 6, fire-clay full of nodules as large as chestnuts; the appearance of these fire-clays, crowded with nodules of iron ore, is very striking; their gnarly, knobby outcrops form long reefs visible by lines of breakers far out to sea.—Clays of various shades 12, blue black $\frac{1}{2}$, red, yellow and green, - - - - - - - - - - - $25\frac{1}{2}$

Sandstone, false-bedded, then in layers 12, becoming clayey 4, blue fire-clay 5, - - - - - - - - - - - - 21

These are the last rocks seen at the north side of the mouth of Great Glace Bay. The whole thickness of rocks measured is as follows:—

North of Little Glace Bay, 471 feet
South " " " " 436 " } in all 907.

Beneath these rocks lie formations of clay (including coal beds, one seven or eight feet in thickness), which form the west end of the long line of sea cliffs running out eastward from the Great Glace Bay bar. A measured section was made of these cliff-rocks also, which I propose to give at another time. It repeats a certain portion of the section given above, with interesting variations.

Our section of 907 feet of rock, commences at the headland in the centre of the synclinal and runs along the coast southward. Commencing at the same headland and running along the coast westward, a similar section may be obtained of the same rocks as they rise from the synclinal in that direction at the same low dip. Such a section would be from Cadougan's Creek, which corresponds to Little Glace Bay, to the mouth of Lingan Bay, which in like manner corresponds to Great Glace Bay. Many interesting variations in the metals would appear from such a comparison. While the general regularity and parallelism is remarkable, there are numerous minor irregularities; some fine instances of false bedding and local deposition; lenticular masses of sand separating adjacent mud-rocks; passages of shales into sandstones, and *vice versa;* gradual coalescing of scattered nodules of clay iron-stone into solid plates, or their gradual pervading of a thick bed of fire-clay, hardening it into so refractory a rock, that its outcrop forms a reef far out to sea. Instances occur of of the splitting of coal-beds. The Lingan bed, for example, has, on the sea-shore, a clay parting of half an inch, which in a quarter of a mile inland, thickens to nine inches; and then, in four hundred yards of gangway continued inland, thickens to nine feet, throwing the upper member of the bed entirely beyond the workings.[2] In this we have probably the explanation of the difference between the abandoned Bridgeport bed, on the south shore of Lingan Bay, and the Lingan bed on the north shore, separated by a wide and gentle anticlinal; the Bridgeport bed being but 7 feet thick, while the Lingan bed is 9.

The described section embodies the productive Coal-measures of the east end of Cape Breton, with five workable beds of coal, one of which can hardly be called workable in this area, whatever may be its character in others. In Mr. Brown's section of the North Sydney Coal-measures, there are enumerated, indeed, thirty-four coal-seams; but only four are said to be of workable thickness: Cranberry Head, 3·8 feet; interval (measuring downwards) 280 feet; Lloyd's Cove, 5·0; interval 730 feet; Main

[2] The *Cook Vein*, at Broad Top City in Pennsylvania, has a sandrock parting two feet thick, between two 2 foot beds of coal. At the present heading of the long drift, this rock, after first disappearing, leaving the bed of coal 6 feet thick, has increased to ten feet of tough rock, between two 6 inch beds of coal. This increase of ten feet takes place without crush in a distance of only three or four yards.

Seam, 6·9; interval 450 feet; Indian Cove, 4·8. Mr. Brown's whole section extends to a depth of 1860 feet, or along 5000 yards at a dip of 7° to the N. 60° E.

Mr. Brown "concludes from the best information in his possession that the *productive* Coal-measures exceed 10,000 feet," but I saw nothing in Cape Breton to justify the supposition. He grants that, "owing to several extensive dislocations, it is impossible to ascertion their total thickness with any degree of accuracy." I can only suggest, with deference to his long experience and acknowledged skill, that the structure of the east coast of Cape Breton has not been regarded from a right point of view, inasmuch as the coal-beds have been represented as members of one area, dipping broadside into the waters of the gulf; whereas, in fact, along that coast, they occur with alternate northeast and southeast dips, forming a series of basin-ends, the bodies of which lie side by side submerged beneath the gulf. The same four or five workable beds, inclosed in the same one or two thousand feet of *productive* measures, appear on shore at the west end of each of these basins. As the dip is commonly gentle, viz: from 4° to 8°, the basins sometimes coalesce; but in one instance at least, that of Cow Bay, the *south* dips are 45°, and the basin is sharp and narrow, greatly resembling the end of one of the anthracite basins of Pennsylvania. As at Sydney, and again at Glace Bay, so here at Cow Bay there are but four workable coal-beds in about 1500 feet of *productive* measures, and they are, no doubt, the Glace Bay beds.[3]

Sir William Logan, Sir Charles Lyell, Prof. Dawson, and other geologists who have described the Coal-measures of Nova Scotia and New Brunswick, agree in assigning to them an almost incredible thickness. "The entire section of the Joggins," writes Sir William Logan, "contains 76 beds of coal and 90 distinct Stigmaria underclays," with "24 bituminous limestones," in "a vertical thickness of 14,570 feet."

When we analyze the eight divisions into which this immense mass has been distinguished, we find them thus constituted:

Nos. 1, 2. Sandstones and shales; drift-trees and erect calamites, - - - - - - - - - -	2267 feet.
No. 3. Sandstones; coal shales; underclays; 22 coal-beds,	2134 "
No. 4. Sandstones and shales, gray; bituminous limestones; 45 coal-beds; shells and fish-scales, - - -	2539 "
No. 5. Sandstones and shales, red; carbonized plants, -	2082 "

[3] The combined thickness of the Lower, Middle, and Upper Coal-measures, as determined by Mr. Jukes, in South Staffordshire, England, is 1810 feet. The thickness of the productive Coal-measures of Leicestershire does not exceed 2500 feet. In most parts of the deep anthracite basins, 2000 feet would be a fair average. In Western Virginia and Pennsylvania, and in the deepest parts of the Mississippi Valley areas, 1500 feet.

No. 6. Sandstones, $\frac{3}{8}$; shales; bituminous limestone; 9 coal-beds; shells and fish-scales, - - - - - -	2240 feet.
Nos. 7, 8. Sandstones, conglomerates, shales, nodular limestones, two beds of gypsum; remains of plants, -	2308 "
Interval, - - - - - - - - - - - -	300 "
Massive limestone with *Prod. Lyelli* and other Lower Carboniferous fossils.	

It is very evident that the Sydney, Glace Bay, or Cow Bay section of less than 2000 feet of productive Coal-measures, can represent but barely one of these divisions, and that it must be either No. 3, or No. 4, or No. 6. Sir William Logan adds, in his resumé, that "Nos, 3, 4, 5, and 6,[4] contain the equivalents of the productive Coal-measures of Pictou and Sydney, and, in part, of the sandstones which separate them from the Lower Carboniferous series." Prof. Dawson describes minutely his own section of "2819 feet of the central part of the Coal Formation,"[5] in approaching which, after describing the lower parts,[6] he says: "We have now, after passing over beds amounting altogether to the enormous thickness of 7636 feet, reached the commencement of the true Coal-measures."[7] By the *true Coal-measures* he means, therefore, Division No. 4 and the lower part of Division No. 3, embracing less than 3000 feet of measures and containing but four coal-beds which can be called workable, the rest being from one inch to eighteen inches thick. In descending order we have:

Nine small seams in a thickness of measures of - -	536 feet.
Main coal seam, 3·6; parting, 1·6; coal, 1·6, - -	5.
Three minute seams in an interval of - - -	75 feet.
Coal, ·3; clay, ·5; Queen's vein, 1·9; shale, 4·4; coal, 1·0,	3.
Ten small seams (largest 1·2) in an interval of - -	762 feet.
Coal, with three clay partings, - - - - -	$2\frac{3}{4}$.
Three small seams in an interval of - - - -	206 feet.
Coal, - - - - - - - - - -	5.
Three small seams in an interval of - - - -	17 feet.
Coal, - - - - - - - - - -	4.
Interval of - - - - - - - - -	32 feet.
Coal and bituminous shale, - - - - - -	5.
Eleven small seams in an interval of - - -	1153 feet.

The aspect of this section resembles those on the east coast of Cape Breton, where Modiolæ and fish-scales are also abundant.

The Albert or Pictou section is said also to contain but five or six seams of coal, two of which are of unusual thickness, as follows; From the surface, down the Success Pit, 73 feet; Main Coal, 39·11 feet thick; Interval, 157 feet; Deep seam, 24·9. Both these coal-beds, however, are far from presenting solid faces

[4] Dawson's Acadia, p. 178. [5] p. 177. [6] p. 127.
[7] Described in Proc. Geol. Soc., x, 1-42.

of coal. On the contrary, they are built up, like the 30 and 60 foot coal-beds of the Anthracite region of Pennsylvania, of many layers separated by underminings. The peculiarity here is that these separations are plates of ironstone, not more than six inches thick, instead of being layers of fire-clay, coal-slate, or sandstone. The structure is certainly peculiar, and convinces us of the quietness of deposit and of the long-continued stability of the sea-level.

But inasmuch as the 60 foot coal at Mauch Chunk, on the Lehigh, is identifiable with the Low Main or Mammoth bed of the Pottsville Basin to the west, and of the Beaver Meadow, Hazleton, Buck Mountain, and Wyoming Basins to the north of it, and through them with still smaller and separated beds further off in the Mahanoy and Shamokin Basins, and even with the bituminous basins of the Alleghany Mountains,—there can not be, *a priori*, a reasonable ground for doubt, that the 25 and 40 foot beds of Pictou are identifiable with 5 and 6 foot beds of New Brunswick on the one side, and with the 8 and 9 foot beds of Sydney on the other.* The paleontological unity of the Low Main coal of the Pittsburg region with the Low Main coal of Eastern Pennsylvania is no longer a matter of discussion. The structural evidence also is coincident and precise. Yet, wider intervals of Devonian and Silurian denudation are to be bridged by the theoretical connection *there*, than are called for between the coal areas of the British Provinces. The general bordering of the sea-coast with coal-beds, and the long and parallel stretches of Carboniferous rocks through the interior, are all cogent arguments for the continuity of the original coal areas, and therefore for the contemporaneity of the remaining portions of the coal-beds. As the same coal-beds which now cap the highest mountains of the Alleghanies in Northern Pennsylvania, and have been swept away over wide intervals of Devonian valleys between them, descend also into the depths beneath the beds of the lowest valleys drained by the Swatara, the Schuylkill, the Lehigh, and the Susquehanna North Branch, so I have no doubt the coal-beds, whose edges we now see only

* To illustrate in a still more striking manner this separation of a large bed into several smaller ones, one has only to examine Mr. Jukes's description of the Thick coal of Dudley, in England, "which, forming at that place *one* solid seam ten yards in thickness, becomes split up into *nine* distinct seams by the intercalation of 420 feet of strata over the northern area of the coal-field." The Main coal of the Warwickshire area is split up, according to Mr. Howell, into *five* beds by 120 feet of intervening strata. The Main coal of Moira is noticed by Mr. Hull as a third instance. (See Hull's Paper on the Carboniferous Strata of England, vol. xviii, No. 70, *Quar. Jour. Geol. Soc.*, p. 139.) Mr. Lesquereux, in his Report on the East Kentucky Coal Field, in the fourth volume of Owen's State Reports, p. 360, gives what he considers sufficient evidence of a similar breaking up of the Low Main Coal of the Pittsburg area into three. This is precisely the normal number of large beds into which the great Mauch Chunk or Mammoth Bed separates throughout the Pottsville-Tamaqua Basin.

along the sea-shore of Nova Scotia, or on the sides of the interior low lands, did once ride over the tops of its metamorphic Devonian mountains, whose summits, crowned with cliffs, opposing anticlinal and synclinal dips, remind the Pennsylvanian geologist, at every view he takes of them, of those mountains on which the coal still lies in fragmentary patches in his native State.

What, then, are the thousands of feet of rocks included in Divisions Nos. 5, 6, 7, and 8 of Logan's great section? In other words, the 7630 feet over which Dawson climbed to reach the bottom of his "true Coal-measures?"

What, I ask in reply, are those wide stretches of low, rolling, arable country, with a red shale soil, which the traveller sees spreading around all the productive coal areas of Cape Breton and Nova Scotia, especially the latter? To the geologist from the West they afford familiar scenery. He can hardly persuade himself, sometimes, that he is not riding through Lykens or Locust or Catawissa or Trough Creek Valleys in Pennsylvania, over the chocolate-colored soils of No. XI.[9] This formation, 5000 feet thick around the southern Anthracite coal-fields, becomes, indeed, thinner and thinner northwestward, until it is but 500 in the Alleghany Mountains, and not more than 50 beneath Pittsburg. But along its thickest line it extends from Alabama to New Jersey, a good thousand miles. It would not be surprising, then, to see it stretching another thousand miles further in the same direction, and spreading undiminished around the coal areas of Nova Scotia.

Division No. 5 of Logan's section consists of red shales and sandstones chiefly, 2012 feet thick. There is no reason why this should not be the representative of Formation No. XI, or of its upper part.

If it be objected that Division No. 6 is in fact a coal system with nine beds of coal and numerous bituminous limestones, the objection becomes an additional argument for the identification. For we see in this No. 6 the reproduction, at this immense distance, of the Lower or False Coal-measures of Virginia, where a *productive* coal system underlies the chocolate shales of Formation No. XI, and not only reappears, with workable beds, in Eastern Kentucky and Middle Tennessee, but projects itself, in a recognizable shape, through Western Indiana nearly to Chicago, and through Middle Pennsylvania nearly to the Delaware River. In fact, Lesquereux pronounces the whole coal of Arkansas to belong to this lower system. It may therefore, very well be found in force in Nova Scotia. Throughout Division No. 6 no bed of respectable size is mentioned. It is an early and imperfect system.

[9] The numbers of Formations, used in these pages, are those originally used in the Reports of the Geological surveys of Pennsylvania and Virginia. Prof. Rogers has since then given them.

The chief objections to the hypothesis above sustained will come (1) from the absence of any general representative for the Millstone grit or Great Basal conglomerate of the True Coal-measures; (2) from the sub-position of Divisions 7 and 8, 2308 feet of sands, pebble-rocks, and limestones; and (3) from the presence at a still lower depth of what seems to be the genuine, massive, Subcarboniferous limestone. To break the full force of these objections, I can only remark, (1) that the Pictou coal-basin *has* a massive conglomerate under its productive Coal-measures, while elsewhere no one formation of the whole Palæozoic System is so variable and unreliable and unidentifiable as Formation XII, the Great conglomerate, technically so called; (2) that Nos. 7 and 8 may be identified with Formation X; and (3) that the Subcarboniferous or Archimedes limestones of the Western United States not only have been subdivided into five separate formations in the Valley of the Mississippi, but wholly thin away and disappear before crossing the Schuylkill and Lehigh Rivers on their way to Nova Scotia. Therefore, although the False or Lower Coal-measures of Virginia and Southwestern Pennsylvania are *overlaid* by limestones with Subcarboniferous fossils, the connection, *as to limestone*, is entirely cut away between them and the Nova Scotia deposits, so that the massive gypseous limestones of Nova Scotia may be at any assignable lower level. This argument is rendered all the more forcible by the fact that gypsum is unknown in the United States, except in one or two anomalous positions, apparently connected with the Lower Silurian limestones, and in the closed basin of Michigan.

Beneath the red shale Formation No. XI, we have, in the southeastern ranges of the Appalachians, nearly three miles' thickness of sedimentary deposits, separable everywhere into three great formations: No. X, white sandstone, 2000 feet, No. IX, red sandstone, 5000 feet, No. VIII, green and olive shale, 8000 feet; the white sandstone including rarely a thin bed of conglomerate here and there, and traces of coal-plants and even thin coal-beds; the red sandstone passing downwards into red shale, and often alternating flinty sandrock with massive mud-rocks even in the upper part; and the olive shale becoming near the base of it rocky, and even mountainous in the region of the Juniata, where a system of thin coal-beds was also developed in the midst of the sandstone and shale. The white sandstone of No. X becomes, in the Alleghany Mountain belt, less than 800 feet thick, and is there characterized by thin-bedded and very irregularly cross-bedded sandstones of a peculiar greenish tint and harsh, rough fracture, weathering to a surface sprinkled with small red dots of peroxyd of iron.

It is not too much to say that a geologist well accustomed to these formations, along their great Appalachian belts of moun-

tain and valley, stretching from the Appalachicola and Alabama Rivers in the South, to the Delaware and Hudson in the North, cannot fail to recognize them and distinguish them anywhere. The *tout ensemble* or *facies* of each is *sui generis*. Fossils may come in afterwards as a satisfactory confirmation; but the eye has already determined the respective formations. Even in the West, where Formation IX has dwindled, like Formation XI, to an insignificant one or two hundred feet, and scarcely separates the green sands of X from the green shales of VIII, the characteristic features of the three formations, although modified and harmonized by the preponderance of the argillaceous element, are still in sufficient contrast to be recognized when fairly seen.

To an eye thus trained among the broad outcrops of the Lower, Middle, and Upper Devonian of the Appalachians, it is evident that the mountains of Cape Breton and the hills of Northern Nova Scotia, surrounding or intervening between the already-mentioned red shale borders of the coal areas, are composed of these formations. True, the anticipation of finding these formations has a tendency to warp the judgment and delude the eye, especially when that anticipation is based upon such a probability as this: that a mass, three miles thick and a thousand miles long, will maintain its thickness (and of course its topographical height and geographical breadth) at least as far along the prolongation of its isometric axis (to use Mr. Hull's new and much-needed term), as will such minor formations as the Coal over it or the Upper Silurian limestones under it. In other words, if analogies between the Nova Scotia and the United States coals compel us to consider them synchronic, if not originally conterminous; and if the Clinton fossils of New York, and even the Dyestone[10] iron ore of Pennsylvania, Tennessee, and Wisconsin, be found at Arisaig, and along a well-defined outcrop in the direction of Truro; surely the Second Mountain, Little Mountain, Orwigsburg Mountain, and Summer Hill, upon the Schuylkill River, must be represented by the Antigonish Mountains of Nova Scotia, and by the Sydney and St. Peter's Range in Cape Breton: and this, whether the Nova Scotia Carboniferous rocks or Subcarboniferous limestones be deposited upon the Devonian conformably or unconformably. The Province is in fact a wide belt of mountains partially submerged; and may have been to some extent in the same condition at the beginning of the Coal era. In the Antigonish Hills we may have principally Formation VIII, while in the country south of the Lake Bras d'Or we may have the full series of VIII, IX, and X. The Arisaig formation, with fossils once thought by Hall and Lyell to be Hamilton and Chemung, and now consid-

[10] Described by Dawson, p. 58, supplementary chapter to *Acadian Geology*, August, 1860.

ered by Hall and Dawson to be indisputably Clinton, although overlaid and concealed along most of its extent by apparently nonconformable Coal measures, gives us a fixed lower limit for the so-called metamorphic hill country of the Province, which makes this hill country necessarily Devonian, or Formations VIII, IX, and X. Even if we object to the term Devonian, and permit the paleontologists to carry down the term Carboniferous, or the term Subcarboniferous, step by step, so as to include first, Formation X, perhaps rightly, and then the genuine Old Red IX, and even, as the effort is in the Western States, to include Formation VIII down to its black shale beds with coal, the change of term will not change the lithology,—the mountains of Nova Scotia must still be the representatives of the Catskill, Mohantongo, Terrace, and Alleghany Mountains of New York and Pennsylvania.

The eye can hardly be mistaken in the features of the roadside banks between Antigonish and Merigonish; the road defiles through hills of VIII. Equally certain is it that the outcrops on the road from St. Peter's to Sydney are of the reddish and greenish rocks of IX and X. The road for forty miles winds along the lake shore, and in and out of ravines descending from a group of parallel mountains of these formations, made parallel by a system of parallel anticlinal and synclinal curves which issue from the lake and throw the mountain dips to the north and to the south alternately, at angles from 5° to 45°. Great rib-plates of flinty sandrock rise to the summit and form tablets with broken cliffs upon the outcrop side, fine objects seen thus against the sky. The mountains at the head of the east arm of the lake, and those on its northern side forming the peninsula, come down upon the shore in the same style, and belong to the same system. On the south side of Miré Bay, in the ravines east of the Gabarus road bridge, there is no mistaking the aspect of masses of slates of No. VIII standing at 45°; nor can one be convinced that he is not riding through a forest grown on a soil of IX, as he is whirled over the fine old road from Miré bridge to Louisburg, although the highest elevation of the plateau is but 350 feet.

Whatever impression the Devonian and Subcarboniferous sediments of Nova Scotia and Cape Breton may make upon a geologist from the Middle States, certainly his wonder will be piqued by striking analogies between the exhibitions of the workable Coal-measures at two such distant places as Sydney and Pittsburg. The resemblance is more than general; it has special points.

At Pittsburg there are about a thousand feet of Coal-measures (to the top coal), with a great bed 8 or 10 feet thick near the top, a 6 foot bed half way down, two small workable beds in

the lower half of the column, and a large bed (4 to 8 feet) at the bottom.

At Sydney (Glace Bay), in like manner, there are about a thousand feet of Coal-measures, with an 8 or 9 foot bed towards the top, a 6 foot bed half way down, two smaller beds in the lower half of the column, and a 7 or 8 foot bed near the bottom.

At Pittsburg, as at Glace Bay, the upper 18 inches or 2 foot of the high Main coal is rejected.

At Pittsburg, as at Glace Bay, the middle 6 foot coal (Upper Freeport of the Alleghany River and Cook Vein of Six Mile Run) is famous for its solid face and excellent quality.

No one should admit that such coincidences furnish a demonstration of identity. But it must not be overlooked that the beds of the Pittsburg area have been traced and identified from end to end of areas with a diameter, in all, of over a thousand miles, even across the denuded interval of Central Kentucky. The expectation may, therefore, be pardoned, not as an amiable enthusiasm, but as a logical inference, that when the fossil groups of the individual beds of Cape Breton shall have been thoroughly studied by Lesquereux and other competent botanists, their identification with the beds of the West may be made somewhat more than possible. The zone of sediment, when taken along its isometric axis, is equal enough over *a priori* incredible distances. Logan and Hunt and Murchison are finding the Quebec group and the Huronian and Laurentian systems in Scotland and Scandinavia, not by fossils, but by aspect. No one doubts the extension of the Millstone grit and the Mountain limestone of England to Pennsylvania. Why should the remarkably homogeneous and continuous Flora of any one of the immensely outspread beds of the United States not be homogeneously continuous to Rhode Island, New Brunswick, and Cape Breton?

One remarkable feature, however, in this resemblance of the two coal columns at Pittsburg and Sydney, must not be forgotten. I refer to the mass of red shales which cap the Glace Bay section. A similar deposit occurs, at a fixed horizon, widely spread over Western Pennsylvania, but *beneath*, not *above*, the High Main coal.

Note on Mr. Lesley's Paper on the Coal-measures of Cape Breton; by J. W. Dawson, Principal of McGill College, Montreal.[11]

The new facts and general considerations on the Nova Scotia coal-field contained in Mr. Lesley's paper, are of the highest interest to all who have worked at the geology of Nova Scotia. I think itmy duty, however, to take exception to some of the statements, which, I think, a larger collection of facts would have induced

[11] This note was read by Professor Lesley before the American Philosophical Society, and is published in the same number of its Proceedings.

Mr. Lesley himself to modify. My objections may be stated under the following heads.

(1.) It is scarcely safe to institute minute comparisons between the enormously developed Coal-measures of Nova Scotia, and the thinner contemporary deposits of the West, any more than it would be to compare the great marine limestones of the period at the West, with the slender representatives of the part of the group to the eastward.

(2.) There is the best evidence that the Coal-measures of Nova Scotia never mantled over the Devonian and Silurian hills of the Province, but were, on the contrary, deposited in more or less separate areas on their sides.

(3.) Any one, who has carefully compared the Coal-measures of the Joggins with those of Wallace and Pictou, must be convinced of the hopelessness of comparing individual beds, even at this comparatively small distance. *A fortiori*, detailed comparisons with Pennsylvania and more distant localities must fail.

(4.) I do not think that any previous observer has supposed that the coal-measures of Eastern Cape Breton represent the whole of the coal formation of Nova Scotia. The "Upper Coal-measures" of my paper on Nova Scotia are certainly wanting, and probably the Sydney coal-field exhibits no beds higher than the middle of No. 4 of Logan's section at the Joggins.

(5.) The whole of the coal-beds at the Joggins belong to the *Upper* and *Middle* Coal-measures. It is quite incorrect to identify No. 6 of Logan's section with the *Lower* Coal-measures. These do not occur at the Joggins, but are found in Nova Scotia, as in Virginia and Southern Pennsylvania, at the base of the system under the marine limestones. The Albert beds are the equivalents of these Lower measures, and not of the Pictou coal. In my paper on the Lower Carboniferous Coal-measures (*Journal of Geological Society of London*, 1858), will be found a summary of the structure of the Lower Coal-measures, as shown at Horton Bluff, and elsewhere. The term "true-Coal-measures," quoted by Mr. Lesley, does not mean in my description, the Middle Coal-measures, but merely that part of them holding the workable coal-seams.

(6.) Whatever may be the value of Mr. Lesquereux's applications of the fossil flora to the identification of coal-seams in the West, I am prepared to state, as the result of an extensive series of observations, still for the most part unpublished, that in Nova Scotia, the flora is identical throughout the whole enormous thickness of the Middle Coal-measures, and that the differences observable between different seams are attributable rather to difference of station and conditions of preservation, than to lapse of time. It is, indeed, true, as I have elsewhere explained, that the assemblages of species in the Lower, Middle, and Upper

Coal-measures may be distinguished; but within these groups the differences are purely local, and afford no means for the identification of beds in distant places.

(7.) I do not desire to offer any opinion on the questions raised by some American geologists, as to the extension of the term Carboniferous to the Chemung group; but I know as certain facts, that the flora of the Lower Coal-measures, under the marine limestones and gypsums of Nova Scotia, is wholly Carboniferous, and that the *flora*, on which alone I consider myself competent to decide, of the Chemung of New York, as now understood by Professor Hall and others, and also of the groups in Pennsylvania named, by Rogers, Vergent, and Ponent (? IX and X of Mr. Lesley), is as decidedly Devonian, and quite distinct from that of the Carboniferous period.[13]

For Mr. Lesley's ability as a stratigraphical geologist, I have the highest respect; and, with reference to the present subject, would merely desire to point out that he may not have possessed a sufficient number of facts to warrant some of his generalizations, on which in the meantime I would, for the reasons above stated, desire geologists to suspend their judgment.

J. W. DAWSON.

McGill College, Montreal, February 18th, 1863."

Mr. Lesley remarked that he read this communication of his friend, Dr. J. W. Dawson, with great pleasure, as it would prevent any mistake about the nature and importance of the discussion, and any undue weight being attached to his own suggestions; that no one was more convinced than himself that there could be no excuse for dogmatism where so little was known, and, therefore, that he had intended rather to suggest than to defend those opinions expressed in his paper, which had drawn down so earnest and valuable a caveat from so high a source. To defend them would require long and systematic researches on the ground, if, even then, the too easily accepted present standpoint of paleontology would not hide the truth from view behind immovable obstacles. So long as apparent specific identity in organic forms continues to be accepted as the supreme test of stratigraphical horizon, discord is inevitable. When paleontology is prepared to return under the mild dominion of her mother, lithology, which she has at least one-half repudiated, geology will advance more rapidly in her work.

Dr. Dawson's first objection is a begging of the very question, whether the Coal-measures of Nova Scotia *are* "enormously developed." That, in one spot of the earth's surface like Nova

[13] See paper on Devonian Flora of Eastern America, *Quar. Jour. Geol. Soc. Lond.*, November, 1862. Also *this Journal*, May, 1863.

Scotia, and that too midway between the great coal areas of America and those of Europe, wherein the thickness of Coal-measures proper range from 2000 to 5000 feet, if they even attain the latter size, there should be an anomalous deposit of 25,000 feet, is incredible.[13] What the great Bohemian paleontologist, by unerring instinct, said to us after our thirty years' war over the Taconic system, *there must be a mistake somewhere*, I must repeat to those who so "enormously develop" the Nova Scotia Coal-measures. And my intention in the paper on Nova Scotia coal was only to suggest one formula on which the error might be discussed. I distinctly repudiated the safety of instituting "minute comparisons." My comparison of the Cape Breton coals and the column at Pittsburg was carefully made in the most general manner, and the resemblance called a coincidence. But the value of the comparison remains; for it affords a new argument in favor of the *family likeness* of those parts of the general Coal-measures of different countries, which have a right to the specific title of "productive coals." The argument also remains good, that, if 2000 feet of Coal-measures in Missouri can be recognized in 2000 feet of Coal-measures in Kentucky, Virginia, and Eastern Pennsylvania, the very same system of beds, bed for bed, being demonstrated first by stratigraphy, and then by paleontology (and such is the fact), why not in Nova Scotia? Even granting (3) that sufficient skill and care and opportunity combined have hitherto failed to identify the coals of the Joggins with those of Wallace and Pictou, there is still hope at the bottom of the box. Before Lesquereux undertook the study of the slack-heap at the mine's mouth, our own identification of individual beds was very imperfect, and the search for a complete system of identification had been abandoned with the same sense of hopelessness. But how is it now? There certainly may be special difficulties in Nova Scotia; there are such at Pottsville, and in Michigan; but they are exceptions which prove the rule, instead of affording an *a fortiori* argument against it.

I have no doubt that some of the Coal-measures of the British Provinces may have been "deposited in more or less separated areas on the sides of the Devonian and Silurian hills," as Dr. Dawson says (2). But I confess to a complete scepticism of the great extent which has been assigned to this unconformability of the Coal-measures upon the lower rocks; first, because most of the Island of Cape Breton, and much of the

[13] We have received a note from Dr. Dawson, written after he had seen the above remarks of Prof. Lesley, in which he says he never claimed any such thickness as 25,000 feet for the Coal-measures proper of Nova Scotia, but that the actual measurements of Sir Wm. Logan, carefully revised by himself, gave at one place the truly enormous thickness of nearly 10,000 feet, and that it is to this that his remarks apply. See Dawson's *Acadian Geology*, pp. 117 and 177.—Eds.

surface of Nova Scotia and New Brunswick are confessedly unstudied and almost unknown; secondly, because the incredible thickness assigned to the Coal-measures throws doubt upon the positions assigned to the non-conformable horizons; thirdly, because the coal-beds themselves stand almost vertical in many places round the shores; fourthly, because the mountains of Nova Scotia, with apparently conformable Carboniferous limestones, have apparently an Appalachian structure and aspect, have suffered vast denudation, exhibit cliff outcrops and section ravines, and may just as well have carried coal upon their original backs, as we can prove that our Tussey, Black Log, Nescopec, Mahoning, Buffalo, Tuscarora, Brush, and other Silurian and Devonian mountains did. There is an immense non-conformable chasm in the column west of the Hudson River, and the Catskill Mountains over it have no coal upon their backs; but the coal comes in regularly enough on them at the Lehigh, (a less distance than from Sydney to St. Peters, or from Pictou to Windsor,) and the unconformability in the Upper Silurian and Devonian has already disappeared.

Dr. Dawson's fourth objection would be good, if I had really "supposed the Coal-measures of eastern Cape Breton to represent the whole of the Coal-measures of Nova Scotia." But I only suggested that they may be the equivalents of the system of *productive Coal-measures;* that is all. Between the Monongahela and the Ohio, our column of productive coals is capped by another of barren shales and soft sandstones of unknown height, by one estimate 3000 feet thick; and part of this column may represent the so-called Permian measures, which, in Kansas, cap conformably the Coal-measures. Having no knowledge of the fossils, I have no desire to oppose the conclusions of Professor Dawson, as to the part of the column of the Joggins to which the Glace Bay coals apply, but hope that his accurate handling of them will secure some certainty about it. It was the grouping of the beds, and not the fossils, which I wished to bring into prominent notice; because the doctrine of isolated basins, when unfounded, or overapplied, is as injurious to lithological truth, as the careless identification of surface aspect may at any moment prove to paleontology. I willingly leave to accomplished paleontologists, like Professor Dawson, the discussion of the grand generalization embodied in his sixth objection; but I may be permitted to believe that it has had its birth in the doctrine of isolated basins, and that the two must stand or fall together. It also seems to me to involve radical inconsistencies; for, if I comprehend it, it asserts, 1. That the flora of the whole coal-measures (25,000 feet?) is identical; that is, the vertical distribution of each and all the plants is complete from the bottom to the top. 2. That, nevertheless, there are differences observa-

ble between different coal-beds. 3. That these are attributable rather to difference of station and conditions of preservation, than to lapse of time; that is, if we could take the beds, each one in its whole extent, and its fossils in their original condition, there would, after all, be no differences observable between different seams. 4. That groups or assemblages of species in the Lower, Middle, and Upper Coal-measures may nevertheless be distinguished; that is, while each and every species may be found occasionally in all parts of the column from bottom to top, yet this happens in such a manner as to group some of them more abundantly, or in certain peculiar proportions in the Lower, others in the Middle, and others in the Upper portions of it. 5. That, after all, however, these groups are not persistent, but differ at different localities, and are as worthless as the specific forms themselves for the identification of a single bed in more than one place.—Is it possible that all this has been made out, or *can* be made out, except in a country of *horizontal* Coal-measures, well opened for study, where the stratification can be established beforehand, and the range of the fossils be made certain?

In conclusion, I would say, that the want of clearly defined and applied names is a drawback to such a discussion. The discussion is, in fact, *initially* one of names, viz: how far down the name Carboniferous must be carried; what are the Lower Coal-measures, &c. But, *in the end*, it is a question of vital importance to the value of the paleontological *imprimatur* upon stratigraphical and structural deductions from field work. Is the discovery of specific forms to keep all our geological *niveaux* in a perpetual mirage-flicker? Are we never to know, from day to day, whether we are at work in Devonian or Carboniferous, in Trias, or Lias? Why not at once obey the marriage law of the weaker sex, and give up our names for our lords? Let geology forget the virgin nomenclature of her youth, and rewrite her books with such titles for her chapters as these: "The Spiriferiferous formation; The Lepidodendriferous formation; The Lower Thecodont; The Middle Baculite; The Upper Pterodactylian formation. Why has this not already been done? Simply because it cannot be done. No paleontologist has yet been bold enough even to propose it. Yet, as I believe, the 25,000 feet of Coal-measures in the British Provinces will be found to be one of the many unconscious *realizations* of this idea, when no one can be found to *nominate* it openly. The whole Paleozoic system, at its thickest place, in southeast Pennsylvania and middle Virginia, is but 35,000 feet. It is not unreasonable then to *suggest*, if not to affirm, that the vast column of so-called Coal-measures in Nova Scotia will take in all that part of the Paleozoic column which has furnished coal, and that is from the top downwards nearly to the Upper Silurian.

ART. XVIII.—*Hydraulics of the Report of Humphreys and Abbot on the Mississippi River;* by Prof. F. A. P. BARNARD. (Continued from p. 37.)

FOR the solution of most problems in practical hydraulics, it is necessary to establish the relations which exist between the cross-section of the stream, its mean velocity, and the slope of its surface. As a basis of this investigation, it is assumed by the authors of this report, as by other writers on the subject, that the condition of uniform motion is expressed by equating the accelerating force with the resistances. One side of the equation presents no difficulty; it is simply the expression for the force of gravity. The other requires consideration.

The authors of the report reject the idea that the cohesion of the particles of the liquid among themselves enters as an element into the resistance of the liquid to motion. They hold that this cohesion is concerned only in determining the distribution of the resistance through the mass; but that the resistance itself is simply the adhesion of the liquid to its bed. It is unnecessary to stop just here to discuss the question by what name it is most fitting that the resistance to flowing water shall be called. It is quite sufficient, if we agree that were the resistances, irregularities, and obstructions to motion, at the surfaces of contact between the water and the earth or the superincumbent air, to be totally annihilated, the whole body of water would descend with a uniformly accelerated velocity, as a solid descends an inclined plane; and there would be no subsurface curves. The resistances therefore come from the perimeter, and must be proportional to it, and to the length of the channel considered. In the perimeter, the results of this survey go to prove that we must include that part which is in contact with the air, as well as that which bounds the cross-section beneath the water. The question how there happens to be a resistance at the surface, and what is the cause or what are the causes producing it, is a question to be considered by itself. For the moment we accept the fact, as experiment has established it. The resistances must then be proportioned to the perimeter and length of channel; and, also, because, when there is no motion, there is no resistance, to some function of the mean velocity at the surfaces in contact. Now, if we put

a=cross-section of the river, W=width,

p=wetted perimeter, $r=\frac{a}{p}$=mean radius, or hydraulic depth,

l=length of channel considered, h=total head or difference of level,
$h_{'}$=part of head balanced against ordinary resistances of the channel,
$h_{''}$=part of head neutralized by bends, and irregularities,

G=specific gravity of the water, g=measure of the force of gravity, $s=\frac{h_l}{l}$=slope of surface expended against ordinary resistances, $\frac{h_n}{l}$=slope expended against bends, &c. V, U, v, b=same values as before,

we shall have, for the accelerating force of gravity, the expression, *Ggals*, and, for the resistances at the perimeter, the expression,

$$l(p+\mathrm{W})\varphi\left(\frac{\mathrm{U}_0\mathrm{W}+\mathrm{U}_r p}{\mathrm{W}+p}\right),$$

which two expressions are to be put equal to each other. In a very large river, W and P are very nearly equal, though p of course always exceeds W. The expression may be simplified by putting them equal, and no appreciable error will result. Also, G and g are constants, and l is a factor common to both sides. Dividing by these, and putting $p=\mathrm{W}$ in the fraction, there will result the equation,

$$as=(p+\mathrm{W})\varphi\left(\frac{\mathrm{U}_0+\mathrm{U}_r}{2}\right);\quad \text{or}\quad \frac{as}{p+\mathrm{W}}=\varphi\left(\frac{\mathrm{U}_0+\mathrm{U}_r}{2}\right).$$

If we substitute the values of U_0 and U_r given above, the equation simplifies itself immediately to the following:[1]

$$\frac{as}{p+\mathrm{W}}=\varphi(0{\cdot}93v-0{\cdot}167(bv)^{\frac{1}{2}});\ \text{which put}=\varphi(z).$$

Let $\frac{a}{p+\mathrm{W}}$ be denoted by $r_,$=the radius of the entire perimeter: the expression then becomes

$$r_,s=\varphi(\cdot93v-167(bv)^{\frac{1}{2}})=\varphi(z).$$

This preliminary equation differs from that which is usually assumed, in two particulars. First, the radius of the entire perimeter, $r_,$ is employed instead of r, the hydraulic mean depth (to which, however, it is in nearly a determinate ratio); and secondly $\varphi(z)$ is used instead of $\varphi(v)$, or a function of the *mean velocity at the perimeter*, instead of a function of the mean velocity of the river itself.

[1] In stating this expression, the authors have committed a sort of mathematical solecism, in arbitrarily changing the sign of the second term, to guard against the subsequent occurrence of what they conceived to be an inconsistency. We shall see that the apprehension was unfounded. The change of this sign, in fact, contradicts the original hypothesis. For the quantity under the symbol φ is the mean of the velocities at the upper and lower boundaries of the mean vertical plane; and this can never, by any possibility, be greater than the mean velocity in the same plane, which is $\cdot93v$; nor even equal to it, except in the case in which the parabola becomes a straight line, and $v=0$. Therefore, since $(bv)^{\frac{1}{2}}$ has been necessarily taken throughout as essentially positive, the written sign before it must be negative.

In order to determine the nature of the function $\varphi(z)$ and the constants which must enter into it, a collection was made of all the available data which had been furnished by the survey, or which could be gleaned from the publications of other observers; embracing thirty examples of area, width, wetted perimeter, maximum depth, mean velocity and slope of streams varying in magnitude from the dimensions of the Mississippi at high water, down to those of a small canal. In regard to slope, it was to be considered that a portion is expended in overcoming the irregularities of the channel and the changes of cross-section: and a portion, in compensating for the loss of living force at bends. It is only what remains after these effects have been subtracted, which constitutes the equivalent of the resistance of a straight and regular channel. The effect of bends must be provided for in an independent formula; and the amount of slope neutralized by them, deducted from the total slope observed. Irregularities and changes of cross-section in the channel are governed by no law, and therefore cannot directly enter into the formula; but they produce a mean effect, which is provided for in the modification which they introduce into the constants which are derived from observation, on the supposition that, after bends have been allowed for, the channel is straight and regular, and the movement in it uniform. The method pursued by most writers, of putting $\varphi(v)=Av+Bv^2$, and then seeking values for the indeterminate coefficients which shall most nearly represent the observations, was tried by the authors of the report, making

$$r_{,}s=Az+Bz^2, \quad \text{or} \quad \frac{r_{,}s}{z}=A+Bz,$$

in which $\frac{r_{,}s}{z}$ and z are co-ordinates in the equation of a straight line; but they found that a straight line would not represent the observations, and that the involution of z produced expressions of troublesome complexity. They then put

$$r_{,}s=Cz^2, \quad \text{or} \quad C=\frac{r_{,}s}{z^2}$$

and plotted the values of C as ordinates to $r_{,}$, s, and v. The plots with $r_{,}$ and v produced irregular curves following no apparent law. That with s was quite regular. It was inferred therefore that C is some function of the slope. After a very long series of trials, with a view to discover this function, the expression

$$C=\frac{s^{\frac{1}{2}}}{195}$$

was adopted, as most satisfactorily fulfilling the required conditions. Substituting this, therefore, in the formula, it becomes

$$z=(195r_{,}s^{\frac{1}{2}})^{\frac{1}{2}}=\left(\frac{195as^{\frac{1}{2}}}{p+\mathrm{W}}\right)^{\frac{1}{2}}.$$

From this are deduced values for each of the variables in terms of the rest (regarding $p+\mathrm{W}$ as a single variable), viz:

$$s=\left(\frac{(p+\mathrm{W})z^2}{195a}\right)^2,$$

$$a=\frac{(p+\mathrm{W})z^2}{195s^{\frac{1}{2}}},$$

$$p+\mathrm{W}=\frac{195as^{\frac{1}{2}}}{z^2}.$$

Instead of $p+\mathrm{W}$, may be put, without appreciable error, for rivers, 2·015 W. Resuming the value of z, viz:

$$z=0{\cdot}93v-0{\cdot}167b^{\frac{1}{2}}v^{\frac{1}{2}}=(195r_{,}s^{\frac{1}{2}})^{\frac{1}{2}},$$

and solving with respect to $v^{\frac{1}{2}}$, we obtain

$$v^{\frac{1}{2}}=-\sqrt{0{\cdot}0081b+(225r_{,}s^{\frac{1}{2}})^{\frac{1}{2}}}+0{\cdot}09b^{\frac{1}{2}},$$

and

$$v=\left(-\sqrt{0{\cdot}0081b+(225r_{,}s^{\frac{1}{2}})^{\frac{1}{2}}}+0{\cdot}09b^{\frac{1}{2}}\right)^2$$

The negative value of the radical is that which it is necessary to take, in order to fulfil the condition that v shall become zero when s is zero.

For rivers, the value of b, as heretofore given, is 0·1856. The term containing it under the radical will have only the value ·0015, and may ordinarily be neglected. The expressions for the several variables will then become

$$v=(0{\cdot}0388-(225r_{,}s^{\frac{1}{2}})^{\frac{1}{2}})^2=\left(0{\cdot}0388-a^{\frac{1}{2}}\left(\frac{225s^{\frac{1}{2}}}{p+\mathrm{W}}\right)^{\frac{1}{4}}\right)^2$$

$$r_{,}=\frac{(v^{\frac{1}{2}}-0{\cdot}0388)^4}{225s^{\frac{1}{2}}} \qquad s=\left(\frac{(v^{\frac{1}{2}}-0{\cdot}0388)^4}{225r_{,}}\right)^2 *$$

If Q represent the amount of discharge per second, then

$$v=\frac{\mathrm{Q}}{a}, \text{ and } a=\frac{\mathrm{Q}}{v}.$$

If Q be given, along with any two of the foregoing variables, the rest may be computed by the help of this equation, unless the two given at the same time are v and a.

In estimating the effect of bends, the authors found the

* In the last two formulæ, the second term of the numerator has the negative sign, where the authors of the Report have made it positive. This difference results from our having chosen not to adopt the change of sign in the value of z, introduced by the authors, as explained in the note on p. 198. The two formulæ above are the only ones in which the original difference of proceeding involves any difference of final values; and as neither of these is employed at all in the subsequent test computations, the discrepancy has here no practical importance.

formula of Dubuat, with a modification of the constant, to represent very nearly the effect deduced from observation. This formula is (with the constant divisor reduced to English feet)

$$h_{,,} = \frac{v^2 \sin^2 \hat{a}}{266{\cdot}3},$$

in which $\sin^2 \hat{a}$ is the sum of the squares of the natural sines of the amount of bending, divided into angles not exceeding 36° or 40°.

Dubuat derived this formula from observation on the flow of water in pipes, in which the cross-section has no such variation as is always observed in the bends of rivers. The value of $h_{,,}$ is therefore too small, or the constant too large. Observations were made by the survey, to determine the amount of slope required to overcome a given bend. These observations were founded on a principle at once simple and ingenious. A level was run from one point of the river to another, several miles distant, embracing between them a bend and a long straight reach. Simultaneous readings of the stand of the river were made at both ends of the reach, and above the bend. If the bend had not existed, the slope in the reach multiplied into the distance by river between the extreme stations, should give the observed difference of level. The fall is always greater as observed than as computed, by the value of $h_{,,}$. From the data obtained by means of such observations, it was ascertained that Dubuat's formula, with the constant 134, would accord very closely with the observations. The authors therefore give, as the expression for the effect of bends,

$$h_{,,} = \frac{v^2 \sin^2 \hat{a}}{134}.$$

A table of the comparative values of $h_{,,}$ as computed by formula, and as obtained by actual measurement, over distances varying from five to nine miles, is given in the report, in which the differences are all very small, and are proportionally smaller as the distance is greater. In the application of the formula to cases in which an actual examination of every bend had not been made, resort was had to the best maps, and an estimate of the amount of bending made by measurement on the map. As a rough test of the correctness of these determinations, an independent formula was constructed, on the principle that the amount of bending between two points must be approximately proportional to the difference of distance between the points, as measured by an air line, and by the river. Denoting this difference in miles by M, it was found that $\sin^2 \hat{a}$ rarely differed essentially from 0·34 M. A series of comparisons somewhat extended, upon stretches of the river varying from three miles to more than eighty miles in length, gave, for the total of the

observed values of $\sin_2 \hat{d}$, 140·81, and for that of the values computed by the last formula, 141·78. The total of the difference, taken, without regard to sign, was 22·83. This, observe the authors, "is given as an illustration that so far from being, as often declared in popular writings, a river without rule or beyond the restraint of law, the Mississippi is in reality controlled by laws which can be expressed in single algebraic formulæ."

We now come to that part of the report which has impressed us most forcibly with the value of the new formulæ, and the merit of the great labor by which they have been wrought out. This consists in an extended series of tests, in which the results of computation according to the formulæ are compared with actual measurements of the quantities computed. As no adequate idea of the severity and thoroughness of these tests can be formed without an inspection of all the data, along with the results deduced from them, we regard it as in justice due to the authors of the report to insert the following tables in full. It is to be observed that, in all cases, the slopes of rivers, as given in the first table, are the slopes as corrected from the measured slopes, by applying the formula for bends. The second table contains *differences* between the values of the computed mean velocity, and the mean velocity actually measured, in each of thirty cases. This table is rendered especially interesting by the comparison which it exhibits between the results given by the new formula for velocity, and those derived from the formulæ laid down by other writers. The following list embaces all these formulæ:

"*Chezy* ...
- (Young's coefficient) ..... $v = 84{\cdot}3(rs)^{\frac{1}{2}}$.
- (Eytelwein's coefficient) ..... $v = 93{\cdot}4(rs)^{\frac{1}{2}}$.
- (Downing's and others' coefficient) $v = 100{\cdot}0(rs)^{\frac{1}{2}}$.

Dubuat ... $v = \dfrac{88{\cdot}49(r\frac{1}{2} - 0{\cdot}03)}{\left(\frac{1}{s}\right)^{\frac{1}{2}} - \mathrm{L}\left(\frac{1}{s} + 1{\cdot}6\right)^{\frac{1}{2}}} - 0{\cdot}086(r^{\frac{1}{2}} - 0{\cdot}03)$.

In which L = common logarithm multiplied by 2·302585.

Girard ... $v = (2{\cdot}69 + 26384\, r\, s)^{\frac{1}{2}} - 1{\cdot}64$

De Prony
- (For canals) ..... $v = (0{\cdot}0556 + 10593\, rs)^{\frac{1}{2}} - 0{\cdot}2357$.
- (For canals and pipes) $v = (0{\cdot}0237 + 9966\, rs)^{\frac{1}{2}} - 0{\cdot}1542$.
- (Eytelwein's coefficient) $v = (0{\cdot}0119 + 8963\, rs)^{\frac{1}{2}} - 0{\cdot}1089$.
- (Weisbach's coefficient) $v = (0{\cdot}00024 + 8675\, rs)^{\frac{1}{2}} - 0{\cdot}0154$.

Young ... $v = \left(\dfrac{rs}{3\mathrm{A}} + \left(\dfrac{\mathrm{B}}{12\mathrm{A}}\right)^2\right)^{\frac{1}{2}} - \dfrac{\mathrm{B}}{12\mathrm{A}}$.

In which $\mathrm{A} = 0{\cdot}0000001\left(413 + \dfrac{1{\cdot}5625}{r} - \dfrac{90}{3r+8} - \dfrac{15}{4r+0{\cdot}0296}\right)$,

$$B=0{\cdot}0000001\left(\frac{900r^2}{r^2+0{\cdot}5}+\frac{1}{(3r)^{\frac{1}{2}}}\left(271{\cdot}25+\frac{6{\cdot}88}{r}+\frac{0{\cdot}0001146}{r^2}\right)\right).$$

Dupuit.... $v=\dfrac{s\,r\,a}{0{\cdot}08\,W}+(0{\cdot}0067+9114\,rs)^{\frac{1}{2}}-0{\cdot}082.$

St. Venant.. $v=106{\cdot}068\,(rs)^{\frac{11}{21}}.$

Ellet..... $v=0{\cdot}64\,(\Delta H)^{\frac{1}{2}}+0{\cdot}04\,\Delta H.$

In which Δ denotes the maximum depth of the stream, and H the fall in water surface in 1 English mile."

Measurements of cross-section, slope, and resulting mean velocity of rivers

No. of observation.	Stream.	Locality.	Date.	Dimensions of cross-section.				Mean velocity	Slope.	Authority.
				Area.	Width	Perimeter.	Max. depth			
				Sq. ft.	*Feet.*	*Feet.*	*Feet.*	*Feet.*		
1	Mississippi river,	Carrolton.	High w. 1851	193,968	2653	2693	136	5·9288	0·0002051	DeltaSurv'y
2	" "	" "	" "	195,349	2656	2696	136	5·8869	0·0001713	" "
3	" "	"	May 31, 1851	180,908	2421	2461	131	4·9338	0·0000342	" "
4	" "	"	June 3, 1851	183,663	2429	2469	132	3·9775	0·0000384	" "
5	" "	Columbus.	May 15, 1858	148,042	2214	2247	88	6·9575	0·0006800	" "
6	" "	Vicksburg.	June 7, 1858	178,137	2729	2779	100	6·9496	0·0006379	" "
7	" "	"	H. w. 1858	179,502	2732	2782	101	6·8245	0·0004365	" "
8	" "	"	Nov. 6, 1858	78,828	2507	2530	63	3·5234	0·0002227	" "
9	" "	"	Dec. 18, 1858	134,942	2556	2589	83	5·5580	0·0003029	" "
10	" "	"	Dec. 24, 1858	150,354	2580	2621	90	6·3186	0·0004811	" "
11	Bayou Plaquemine,	Near Upper mouth.	Mar. 12, 1851	5,560	292	303	28	5·1979	0·0020644	Mr. C. Ellet.
12	" "	" " "	Jan. 16, 1859	4,259	268	278	24	3·9589	0·0014372	DeltaSurv'y
13	" La Fourche,	" " "	H. w. 1851	3,728	223	238	27	3·0765	0·0004468	" "
14	" "	" " "	May 6, 1851	3,025	223	232	24	2·8430	0·0003731	" "
15	" "	" " "	May 7, 1851	2,957	223	231	24	2·8069	0·0003655	" "
16	" "	" " "	May 8, 1851	2,868	223	230	23	2·7894	0·0004384	" "
17	C. & O. canal feeder	Near Georget'n, D.C.	Nov. 26, 1859	121	23	32·7	7·6	3·0523	0·0003851	" "
18	" "	" "	Nov. 28, 1859	119	23	32·5	7·5	2·7227	0·0003851	" "
19	Ohio River,	Point Pleasant.	Nov. 20, 1858	7,218	1073	1074	8	2·5152	0·0006634	Mr. C. Ellet.
20	River Haine,	France.	1782	248·5	48	50·5	8?	2·4947	0·0016534	M. Dubuat.
21	"	"	1782	306·4	50·5	53·4	9?	2·5579	0·0015563	"
22	Canal,	England.		50	18	20·6	4	1·1336	0·0006313	Mr. Watt.
23	River Rhine,	Byland.	June— 1812	19,135	1155	1163	20	3·5749	0·0009769	Krayenhoff.
24	" "	Pannerden.	" "	6,304	557	563	17·9	3·2706	0·0009986	"
25	" Waal,	Upper mouth.	" "	14,782	1328	1334	17·9	3·1648	0·0010438	"
26	" Rhine,	Below the Yssel.	" "	5,341	700	704	12·9	2·9167	0·0011744	"
27	" Yssel,	Upper mouth.	" "	1,980	321	324	9·9	2·7727	0·0011657	"
28	" Tiber,	Rome.	June— 1821	2,835	243	249	15	3·4132	0·0013061	M. Buffon.
29	" Neva,	Russia.	June— 18—	43,461	1218	1227	50	3·2396	0·0001989	M. Destrem.
30	" Great Nevka,	"	June— 18—	15,554	881	893	21	2·0486	0·0001487	" "

Tests of the several formulæ for mean velocity.

Number of observations.	Chezy's formula with coefficient of			Dubuat's formula.	Girard's formula.	De Prony's formula with coefficients.				Young's formula.	Dupuit's formula.	St. Venant's formula.	Ellet's formula.	New formula.
	Young.	Eytelwein	Downing and others.			For canals.	For pipes and canals.	By Eytelwein.	By Weisbach.					
	Feet.	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*
1	+2·6888	+2·3390	+2·0854	+3·1820	+1·1140	+2·2017	+2·2430	+2·3974	+2·3644	+2·6547	+1·0536	+2·4381	+2 8837	+0·0385
2	+2·9167	+2·5961	+2·3636	+3·4374	+1·5736	+2·4887	+2·5204	+2·6584	+2·6206	+2·9000	+1·4629	+2·7002	+3·1500	+0·2425
3	+2·6973	+2·5530	+2·4484	+3·3542	+2·6207	+2·6208	+2·5978	+2·5378	+2·5725	+2·7822	+2·3651	+2·6534	+2·9552	+0·2593
4	+2·5522	+2·3984	+2·2868	+3·2073	+2·4188	+2·4572	+2·4369	+2·4820	+2·4182	+2·6320	+2·1732	+2·5009	+2·8230	+0·0658
5	+1·3152	+0·7061	+0·2643	+1·5090	−2·3975	+0·3004	+0·4281	+0·7288	+0·7388	+1·1238	−3·0950	+0·7159	+2·0962	−0·8093
6	+1·5591	+0·9772	+0·5552	+2·7722	−1·9246	+0·5998	+0·7184	+1·0038	+1·0091	+1·3844	−0·0904	+0·9996	+1·8883	−0·4602
7	+2·3506	+1·8677	+1·5174	+2·6733	−0·3106	+1·5929	+1·6784	+1·9078	+1·8968	+2·2359	−0·4739	+1·9298	+2·8054	+0·0713
8	+1·3028	+1·0631	+0·8893	+1·6224	+0·5812	+1·0377	+1·0434	+1·1361	+1·0853	+1·3343	+0·8166	+1·1737	+1·4852	−0·3978
9	+2·2085	+1·8469	+1·5847	+2·5718	+0·5389	+1·6975	+1·7427	+1·9037	+1·8726	+2·1690	+0·8041	+1·9437	+2·6953	+0 0430
10	+1·8902	+1·4121	+1·0654	+2·1721	−0·7304	+1·1425	+1·2263	+1·4530	+1·4411	+1·7785	−0·6254	+2·0143	+2·3442	−0·1982
11	+0·0094	−0·5507	−0·9569	−0·1965	−3·2932	−0·9055	−0·7942	−0·5211	−0·5194	−0·1556	−1·4981	−0·5187	+0·4408	−0·0448
12	+0·0033	−0·4238	−0·7334	−0·0583	−2·1974	−0·6407	−0·5738	−0·3760	−0·3963	−0·0801	−0·8769	−0·3435	+0·4993	−0·3871
13	+0·8434	+0·6023	+0·4275	+1·0077	+0·1116	+0·5756	+0·5817	+0·6751	+0·6245	+0·8751	+0·4812	+0·7129	+1·2065	−0·2076
14	+0·9837	+0·7829	+0·6374	+1·1533	+0·5428	+0·7964	+0·7900	+0·8069	+0·8040	+1·0392	+0·7353	+0·8921	+1·2623	+0·0213
15	+0·9835	+0·7866	+0·6439	+1·1535	+0·5695	+0·8039	+0·7963	+0 8651	+0·8076	+1·0613	+0·7447	+0·8954	+1·2442	+0·0304
16	+0·8184	+0·6056	+0·4513	+0·9686	+0·2926	+0·6072	+0·6044	+0·6821	+0·7270	+0·8657	+0·5499	+0·7156	+1·0997	−0 0768
17	−1·2535	−1·7162	−2·0517	−1·7040	−3·7470	−1·9699	−1·8912	−1·6733	−1·6876	−1·3746	−1·9100	−1·6470	−1·4773	−0·0709
18	−1·5406	−2·0008	−2·3346	−1·9857	−4·0141	−2·2520	−2·1741	−1·9576	−1·9723	−1·6603	−2·1895	−1·8953	−1·7497	−0·3594
19	+0·4038	+0·1759	+0·0106	+0·6998	−0·2313	+0·1623	+0·1643	+0·2504	+0·1977	+0·4406	+0·1520	+0·2864	+1·0867	+0·0298
20	+0·0901	−0·1694	−0·3577	+0·0453	−0·7802	−0·2148	−0·2028	−0·0990	−0·1467	+0·1054	−0·2003	−0·0593	+0·5240	+0·0257
21	+0·0364	−0·2358	−0·4333	−0·0038	−0·9301	−0·2940	−0·2779	−0·1671	−0·2128	+0·0447	−0·2847	−0·1265	+0·5194	−0·0886
22	+0·0901	−0·0226	−0·1043	+0·1478	+0·1788	+0·0737	+0·0425	+0·0655	−0·0040	+0·1871	+0·0257	+0·0684	+0·3413	−0·1965
23	+0·1952	−0·1696	−0·4342	+0·2315	−1·5005	−0·3224	−0·2761	−0·1133	−0·1439	+0·1506	−0·5043	−0 0736	+1·1067	−0·4809
24	+0·4577	+0·1534	−0·0673	+0·4877	−0·7571	+0·0627	+0·0891	+0·2179	+0·1774	+0·4492	+0·0070	+0·2597	+1·0020	−0·0544
25	+0·2978	−0·0117	−0·2361	−0·2478	−0·9577	−0·1078	−0·0797	−0·0520	+0·0125	+0·2862	−0·1620	+0·2895	+0·8310	−0·1808
26	+0·4004	+0·1288	−0 0681	+0·4037	−0·5617	+0·0712	+0·0871	+0·1976	+0·1518	+0·4107	+0·0629	+0·2382	+0·8733	+0·0973
27	+0·5513	+0·3115	+0·1376	+0·5584	−0·1711	+0·2860	+0·2918	+0·3845	+0·3337	+0·5803	+0·2855	+0·4221	+1·0048	+0·2993
28	+0·4503	+0·1305	−0 1015	+0·4294	−0·8867	+0·0238	+0·0553	+0·1928	+0·1587	+0·4315	−0·0107	+0·2347	+0·9410	+0·1378
29	+1·3598	+1·1579	+0·0115	+1·4974	+0·9109	+1·1702	+1·1641	+1·2357	+1·1790	+1·4133	+0·9731	+1·2671	+1·8573	−0 4976
30	+0·6919	+0·5455	+0·4393	+0·9601	+0·6026	+0·6112	+0·5888	+0·6300	+0·5649	+0·7771	+0·5348	+0·6463	+1·1609	−0·5191
Sum.	32·9420	28·4411	26·6988	40·4417	37·4472	28·0905	28·1506	29·5258	28·8412	33·3834	25·1488	33.6619	45·3547	6·3920

In order to understand the signs prefixed to the numbers in the foregoing table, it must be observed that the authors have tabulated the differences as *corrections*, not as *errors*. That is, each number must be applied to the result of the particular computation to which it relates, with the sign as written. Thus, the first number in the Chezy-Young formula, viz: +2·6888, indicates that this amount must be added to the value of v which the formula gives, in order to make it equal to the observed velocity, 5·9288. The formula gives 3·2400, and 3·2400+2·6888 =5·9288. This mode of exhibiting results, though it makes the comparative error striking, fails to convey an adequate impression of the *comparative approach to truth*, which is a different, and practically more important thing. Let us take, for illustration, the first four examples, with the results by several of the old formulæ and the new.

	1.	2.	3.	4.
Vel. observed,	5·9288	5·8869	4·0338	3·9775
Chezy-Young,	3·2400	2·9702	1·3365	1·4253
Dubuat,	2·7468	2·4495	0·6796	0·7702
Girard,	4·8148	4·3133	1·4131	1·5587
Prony-Eytelwein,	3·5314	3·2285	1·3960	1·4955
Prony-Weisbach,	3·5644	3·2663	1·4613	1·5593
Young,	3·2741	2·9869	1·2516	1·3455
St. Venant,	3·5907	3·1867	1·3804	1·4766
Ellet,	3·0451	2·7369	1·0786	1·1545
Humphreys and Abbot,	5·8903	5·6444	3·7745	3·9117

Thus, by comparing the actual velocities obtained by the different methods, it will be seen that most of the results are so far from the truth as to make them of little practical value; while the approach by the new formula is so near, that the difference is as likely to be due to errors of observation as to those of method.

There is another particular in which the table will not fail to attract notice. It is, that the old formulæ give better results upon rivers of moderate size, as upon the bayous, the Haine, the Rhine, the Tiber, &c., than upon the Mississippi; though upon the Mississippi itself, their results show great discrepancies. There is, however, one curious exception in the case of small streams. Numbers 17 and 18 are examples upon the feeder of the Chesapeake and Ohio Canal near Washington. The following are the results:

	1.	2.		1.	2.
Vel. observed,	3·0323	2·7227	Prony-Weisbach,	4·7199	4·7050
Chezy-Young,	4·2858	4·2633	Young,	4·4069	4·3830
Dubuat,	4·7363	4·7084	St. Venant,	4·6793	4·6180
Girard,	6·7793	6·7368	Ellet,	4·5096	4·4724
Prony-Eytelwein,	4·7056	4·6803	Humphreys and Abbot,	3·1032	3·0821

The old formulæ all give here a velocity largely in excess; whereas in large streams they are almost invariably in deficiency. The new formula represents these cases with as close an approach to observation as any others. The explanation of the anomaly is not obvious. The example of nearest general agreement of results, appears to be the small river Haine, No. 20, which gives the following:—

Vel. observed,	2·4947	Prony-Weisbach,	2·6414
Chezy-Young,	2·4046	Young,	2·3893
Dubuat,	2·4494	St. Venant,	2·5540
Girard,	3·2749	Ellet,	1·9707
Prony-Eytelwein,	2·5937	Humphreys and Abbot,	2·4690

If we examine the numerical ratio between the sums of the errors of the several formulæ in these thirty cases taken without regard to sign, as given in the table, to the sum of the observed velocities (115·4847), we shall find it to vary from twenty-two per cent for the formula of Dupuit, to thirty-nine per cent for that of Ellet. The formula of Humphreys and Abbot gives five and a half per cent. If we take the algebraic sum of these errors, this last ratio is reduced to three per cent; which is the tendency, as shown by this table toward excess. Examining the other formulæ in the same way, we shall see that they are all in deficiency, with the exception of Girard, who leans on the side of excess to the extent of eleven and a half per cent. The Chezy-Eytelwein formula gives a ratio of twenty-five per cent when the arithmetical sum of the errors is compared with the sum of the velocities; the Chezy-Downing formula gives twenty-three per cent on the same comparison. In these cases the algebraic sum of the errors shows a tendency to deficiency of fifteen and a half per cent for the first, and nine and a half for the second. Of all the old formulæ, the Dupuit appears to be the best; for the arithmetical sum of its errors bears the least ratio of all of them to the sum of the velocities; and the opposite errors, in these examples at least, almost exactly balance.

The second method employed by the authors of the report, to test the accuracy of their formulæ, consisted in computing the differences of level between points of the river distant from each other, in regard to which this difference had been ascertained by measurement. The same computation was made by Mr. Ellet's formula also, the results being introduced into the table along with those derived from the new formulæ, for the purpose of comparison. No computations were made in this case from the other formulæ, their large errors already showing their inapplicability to natural streams. An exception was made in favor of Mr. Ellet's, because it had been expressly designed for rivers. The present test applies equally to the bend formula, and to that for mean velocity. The following table embraces both data and

Tests of the formulæ for slope.

Stream.	Level-stations.	Date.	Area.	Width.	Perimeter.	Maximum depth.	Discharge.	Measured between level-stations			Elliot's formula.		New formula.			
								Sin δ.	Distance.	Fall.	Computed fall.	Error.	$h_{,}$	$h_{,,}$	Computed fall, or $h_{,}+h_{,,}$	Error.
			Sq. ft.	*Ft.*	*Ft.*	*Ft.*	*Cu. Ft.*	*Ft.*	*Miles.*	*Ft.*	*Ft.*	*Ft.*	*Ft.*	*Ft.*	*Ft.*	*Ft.*
Mississippi R.	Ft. St. Philip and B. La Fourche.	H. w. 1851	199,000	2,470	2,510	129	1,150,000	21·60	156·00	20·7	50·9	-20·2	12·9	5·4	18·3	+ 2·4
" "	" " " " " "	L. w. "	163,000	2,250	2,290	114	250,000	21·60	156·00	0·9	6·2	- 5·3	0·1	0·4	0·5	+ 0·4
" "	B. La Fourche and Red river.	H. w. "	200,000	3,000	3,035	113	1,200,000	15·39	122·60	23·7	47·7	-24·0	17·0	4·1	21·1	+ 2·6
" "	" " " " " "	L. w. "	100,000	2,750	2,770	78	250,000	15·39	122·60	3·7	16·6	-12·9	1·8	0·7	5·5	+ 1·2
" "	Red river and Arkansas river.	H. w. 1858	199,000	4,080	4,115	96	1,200,000	56·50	373·00	112·0	172·1	- 60·1	98·4	15·3	113·7	- 1·7
" "	" " " " " "	L. w. "	54,000	3,060	3,070	56	200,000	56·50	373·00	114·0	92·4	+21·6	117·7	5·8	123·5	- 9·5
" "	Arkansas river and Ohio river.	H. w. "	191,000	4,470	4,510	87	1,175,000	47·33	408·00	160·0	214·2	- 54·2	151·7	13·4	165·1	- 5·1
" "	" " " " " "	L. w. "	45,000	3,400	3,410	49	150,000	47·33	408·00	159·0	121·2	+37·8	141·1	3·8	144·9	+14·1
B. Plaquemine.	Plaquemine and Indian Village.	H. w. 1850	5,500	300	318	31	33,500	8·63	8·33	20·0	12·1	+ 7·9	18·0	2·4	20·4	- ·04
" "	" " " " " "	March 12, '51	5,120	300	315	30	29,000	8·63	8·00	17·9	10·7	+ 7·2	14·9	2·1	17·0	+ 0·9
" "	" " " " " "	H. w. "	5,700	300	320	32	35,000	8·63	8·33	20·0	11·9	+ 8·1	17·4	2·4	19·8	+ 0·2
B. La Fourche.	Donaldsonville and Lockport.	" "	3,640	230	245	25	11,500	11·77	55·50	11·9	27·7	-15·8	12.6	0·9	13·5	- 1·6
" "	" " " "	April 11, '58	3,567	221	237	26	9,700	11·77	55·50	8·0	25·7	-17·7	6·8	0·7	7·5	+ 0·5
Sum,										671·8	800·4	302·8			367·8	40·6

results. The data were not in all the cases known with equal degrees of exactness; but the small ratio of the errors to the distances on which the computations severally depend is not only satisfactory, but even surprising. In the last example but one, the error is regarded by the authors, as having been probably in great measure occasioned by the occurrence of crevasses between the points observed. The error which is largest in absolute amount is that between the Arkansas and Ohio rivers at low water, which is regarded as possibly due to sand bars.

The final test, and, as it seems to us, the most satisfactory of all, consists in the application of the new formula to the solution of the important question, how much will the level of a river be raised at a given locality, at which the cross-section and discharge are known, by any given definite increase of the discharge? In investigating this question, it is commonly assumed that the slope of the river is unaltered by the increased volume of discharge. But, as this assumption is not true, the results which are deduced from it are equally erroneous. In order to introduce the variation of slope, or the new slope produced by the addition to the volume, as an element in the computation, it was necessary to ascertain, if possible, by observation, the law which regulates the change.

The level of the water at the mouth of a river is not sensibly affected by a flood. For a certain distance up the course of the stream, the effect upon the slope produced by a rise in the river of a definite amount, will be equal to the total rise divided by the distance to the mouth. But, in general, an addition to the volume of waters produces a swell which passes down the stream like a great wave, so that the level may be actually falling near the head of the valley, when, at points lower down, it has not yet begun to rise. It is therefore evident that the same stand of the river is not always accompanied by the same slope, at any given point of observation, unless it be near the mouth.

From gauge observations, it appears that the form of the wave is tolerably regular, and that the daily *change of slope* is nearly the same for the same stand of the river in rising and falling. It is evident that observations on the passage of the great flood waves may be best conducted in the upper parts of the valley; inasmuch as the wave in its progress down the river tends, from the greater slope on the lower side, to spread itself over a wider and wider base, and loses therefore in the degree of its convexity. Columbus, Kentuky, was, on this account, first selected for study. The cross-section, perimeter, width, gauge-level and discharge of the river were determined for different dates during the progress of each of six marked rises of the river, and the corresponding slopes computed from the formula for those dates.

The *differences of slope* measured, of course, the change produced by the increased volume of discharge.

In the endeavor to ascertain the law of change, the slopes were first plotted as abscissæ, to the gauge-readings as ordinates; and straight lines were drawn connecting the points representing the top and bottom of each rise. These lines were not parallel, showing that the rate of increase of slope varies for different rises. In the further study of their relations, it was discovered that the difference of slope divided by the rise is the abscissa of a curve sensibly parabolic, in which the gauge-reading at the top of the rise, measured from low water mark, is the corresponding ordinate. Or, if x denote the rise, e the primitive gauge-reading, and $e+x$ the gauge-reading at flood; also, if $s_{,}$ and $s_{,,}$ represent the primitive slope and the slope at flood, then the following equation will be true:—

$$\frac{s_{,,}-s_{,}}{x}=\frac{1}{2\mathrm{P}}(e+x)^2, \quad \text{or} \quad s_{,,}-s_{,}=\frac{1}{2}\mathrm{P}(e+x)^2x.$$

The value of $\frac{1}{2\mathrm{P}}$ is to be determined by dividing $s_{,,}-s_{,}$ (of both which slopes the values are deduced, as just stated, by the formula, after the observations have determined the cross-section, discharge, perimeter, and rise of the river) by $(e+x)^2x$. For the same locality it is found to be constant; but it is different at different points in the length of the river.

If now we put $a_{,}$ $\mathrm{Q}_{,}$ $p_{,}$ $\mathrm{W}_{,}$ $v_{,}$ for the cross-section, discharge, perimeter, width, and mean velocity of the river in the primitive stage, and $a_{,,}$ $\mathrm{Q}_{,,}$ $p_{,,}$ $\mathrm{W}_{,,}$ and $v_{,,}$ for the same quantities after the rise; and if, in estimating the increased perimeter of the river occasioned by the rise, we neglect, as we may safely do for a large stream, the inclination of the banks, the new perimeter will be equal to the primitive perimeter increased by $2x$, and we shall have

$$\frac{a_{,,}}{p_{,,}+\mathrm{W}_{,,}}=\frac{a_{,}+\mathrm{W}_{,}x}{p_{,}+\mathrm{W}_{,}+2x}$$

Also, as these denominators are equal and numerators also, we shall have

$$a_{,,}=a_{,}+\mathrm{W}_{,}x; \quad \text{or} \quad a_{,,}v_{,,}=\mathrm{Q}_{,,}=a_{,}v_{,,}+\mathrm{W}_{,}v_{,,}x$$

and

$$x=\frac{\mathrm{Q}_{,,}-a_{,}v_{,,}}{\mathrm{W}_{,}v_{,,}}$$

Now, if the quantities $a_{,,}$ $\mathrm{Q}_{,,}$ $p_{,,}$ $\mathrm{W}_{,,}$ $v_{,}$ (with $z_{,}$ which is its function) be given, and it be required to know how much the river will rise if $\mathrm{Q}_{,}$ be made $\mathrm{Q}_{,,}$, the problem may be solved, and higher equations avoided, by an easy process of trial and error. Let $s_{,}$ be first computed from the formula

$$s_{,}=\left(\frac{(p_{,}+\mathrm{W}_{,})z_{,}^2}{195a_{,}}\right)^2.$$

Then, assuming some definite value for x, obtain the numerical values of $\frac{a_{,,}}{p_{,,}+W_{,,}}$ and $s_{,,}$; the former from the equation given just above, and the latter from the equation,

$$s_{,,}=s_{,}+\frac{1}{2P}(e+x)^2x,$$

in which the reciprocal of the parameter has the value belonging to the locality. This being done, $v_{,,}$ may be obtained from the equation for mean velocity already given, viz:

$$v_{,,}=\left(0{\cdot}0388-\left(225s^{\frac{1}{2}}\frac{a_{,,}}{p_{,,}+W_{,,}}\right)^{\frac{1}{4}}\right)^2;$$

and with this value of $v_{,,}$ a value of x may be formed from the equation just found;

$$x=\frac{Q_{,,}-a_{,}v_{,,}}{W_{,}v_{,,}}.$$

If this last value agrees with the assumed value, the problem is solved. If not, a new supposition must be made. But, as the true value always lies between the two erroneous values—that is, between the assumed one and the computed one—the approximation will be rapid. This method has been applied by the authors of the report to the calculation of many rises in the river, of which the particulars are given in the following table. The results are compared with calculations for the same rises from the formula of Mr. Ellet. The symbols Δ, and L in the table belong to Mr. Ellet's formula, the manner of employing which it is not necessary here to explain.

The only criterion by which it is possible to judge of the value of hydraulic formulæ, is the degree of their accordance with direct observation. We have no principles of positive science, to which, in forming such estimates, we can confidently or safely trust. Were it otherwise, we should long since have had formulæ, concerning the truth of which there would be no room for doubt. But science is not in possession of the material for the construction of such expressions. It can only indicate certain variables which must enter into them; as to the manner in which they shall enter, or whether they are all that affect the case, it is silent. We do not know the physical law of resistance opposed to the movements of a fluid by the surfaces which confine it, nor does it yet appear how we can know it. And so long as it is a fact that all the postulates of theory, and all the resources of analysis, are powerless to tell us what amount of force will be consumed in driving a liquid, with a given velocity, into the mouth of a tube, or through the simplest orifice that can be made in the side of the containing vessel, we may well regard a problem affected by all the complex conditions which

Tests for the formulæ for oscillation caused by variation in discharge.

Locality.	Date.	e	Δ_t	L	a_t	W_t	p_t	Q_t	$Q_{t'}-Q_t$	True oscillation.		Computed oscillation.			
										Kind.	Am't.	Ellet's formula.		New formula.	
												x	Error.	x	Error.
		Feet.	*Feet.*	*Miles.*	*Sq. feet.*	*Feet.*	*Feet.*	*Cubic feet.*	*Cubic feet.*		*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Feet.*
Columbus - - -	Dec. 3 to Dec. 21, 1857	12·7	62	1076	92,730	2102	2115	220,000	+940,000	Rise.	25·3	72·9	−47·6	27·0	−1·7
	Dec. 11 to Dec. 16, "	25·8	75	1076	120,660	2160	2204	691,630	+445,970	Rise.	12·2	20·8	− 8·6	13·8	−1·6
	Mar. 17 to Mar. 28, 1858	25·4	75	1076	119,780	2157	2200	590,000	+530,000	Rise.	15·0	27·6	−12·6	15·0	0·0
	April 11 to April 26, "	24·4	74	1076	117,630	2157	2200	570,000	+690,000	Rise.	18·7	35·1	−16·4	18·7	0·0
	May 7 to June 22, "	32·0	81	1076	134,180	2199	2242	776,550	+606,530	Rise.	14·6	26·6	−12·0	14·7	−0.1
	June 3 to June 22, "	42·3	92	1076	156,500	2218	2252	1,160,970	+222,110	Rise.	4·3	8·2	− 3·9	5·2	−0.9
	July 17 to July 27, "	25·3	75	1076	119,580	2157	2200	424,530	+240,900	Rise.	6·6	17·9	−11·3	6·6	0·0
	Oct. 30 to Nov. 8, "	9·8	59	1076	86,670	2073	2086	143,710	+298,280	Rise.	11·9	37.8	−25·9	11·6	+0·3
Vicksburg - - -	March 6 to March 30,"	29·1	82	487	127,630	2522	2545	670,550	+438,880	Rise.	13·8	18·8	− 5·0	13·3	+0·5
	Mar. 10 to Mar. 23, "	30·9	84	487	132,200	2535	2560	748,200	+199,260	Rise.	6·1	8·5	− 2·4	6·3	−0·2
	Mar. 20 to Mar. 30, "	34·5	87	487	141,350	2558	2585	841,570	+267,860	Rise.	8·4	10·5	− 2·1	7·4	+1·0
	April 19 to June 26, "	45·4	98	487	169,500	2660	2698	1,105,000	+125,900	Rise.	2·9	4·4	− 1·5	2·8	+0·1
	Aug. 6 to Aug. 26, "	45·0	98	487	168,600	2658	2698	1,086,400	−372,340	Fall.	11·6	15·0	− 3·4	11·2	+0·4
	Aug. 26 to Sept. 1, "	33·4	86	487	138,530	2550	2575	714,060	−173,140	Fall.	6·7	8·9	− 2·2	6·7	0·0
	Nov. 3 to Nov. 15, "	8·7	61	487	77,360	2420	2430	236,000	+359,000	Rise.	16·8	25·7	− 8·9	17·6	−0·8
	Dec. 10 to Dec. 13, "	17·0	70	487	97,580	2455	2475	399,920	+117,700	Rise.	5·6	7·3	− 1·7	5·9	−0·3
Carrollton - - -	Feb. 17 to Feb. 23, 1851	6·3	128	121	164,170	2324	2355	534,780	+335,220	Rise.	5·2	9·8	− 4·6	4·5	+0·7
	Feb. 19 to March 1, "	8·2	130	121	168,840	2338	2368	630,000	+382,570	Rise.	5.2	11·0	− 5·8	4·7	+0·5
	Feb. 20 to Feb. 25, "	9·0	131	121	170,800	2344	2374	670,770	+239,130	Rise.	3·0	7·0	− 4·0	3·1	−0·1
	Feb. 25 to March 21, "	12·0	134	121	177,900	2364	2398	909,900	+229,800	Rise.	3·1	6·2	− 3·1	3·0	+0·1
	April 17 to May 9, "	14·8	136	121	183,800	2378	2416	1,065,000	−181,000	Fall.	1·8	5·1	− 3·3	3·2	−1·4
	May 27 to July 25, "	9·9	132	121	173,000	2350	2380	652,330	+222,670	Rise.	3·0	6·5	− 3·5	2·5	+0·5
	July 31 to Aug. 15, "	12·4	134	121	178,810	2367	2401	845,000	−175,610	Fall.	2·6	5·3	− 2·7	2·9	−0·3
	Jan. 7 to Jan. 17, 1852	0·8	122	121	152,000	2287	2312	310,000	+220,000	Rise.	4·3	7·2	− 2·9	4·4	−0·1
										Sum.	208·7	404·1	195·4	212·1	11·6

modify the flow of water in a natural channel, as practically beyond their reach. Hydraulic formulæ must, accordingly, from the nature of the case, be to a great extent empirical; and the highest degree of theoretic plausibility which such a formula may bring to recommend it, can at best only serve as an encouragement to us to try it, in order that we may ascertain how far it may truly represent nature. The experience gathered in such past trials has not, however, been of a nature to render the encouragement a very solid ground of hope for a favorable result.

The test then of actual trial is that to which we must bring at last all theorems in hydraulics; and our judgments of their merits will be regulated by the manner in which they stand this test. This is a principle which the authors of the report before us seem to have fully recognized; and the thoroughness with which they have applied it to their own formulæ is without any past example in the history of such investigations. We think them, therefore, fully justified in the modest claim with which they conclude this part of their labor, viz., that these formulæ are "entitled to the confidence of practical men."

Art. XIX.—*On Inhalation of Nitroglycerine;* by John M. Merrick, Jr.

Various experiments have been made by different observers[1] upon the action of nitroglycerine or glonoine upon the animal economy—the nitroglycerine, or its solution in alcohol, being administered by dropping it upon the tongue—the effects which have been noticed being generally acceleration of the pulse, headache and prostration, and in peculiarly susceptible persons, these symptoms greatly aggravated.

These experiments, though somewhat contradictory, are very interesting, both from a chemical and a toxological point of view, but do not touch upon one matter, viz: the effects of the inhalation of the vapor of glonoine—a subject to which considerable interest must attach itself when we consider the rapidity with which the symptoms develop themselves when only a fraction of a drop is placed on the tip of the tongue.

In preparing a quantity of nitroglycerine in 1859, I met with an accident, the result of which exhibits in a very marked and satisfactory manner the toxical properties of this curious substance, and shows the necessity for extreme caution in handling it, especially when mixed with a volatile and inflammable solvent, as alcohol or ether.

[1] Vide Braithwaite's Retrospect of Practical Medicine, part xxxvii, p. 294.

The nitroglycerine was prepared by allowing pure glycerine to drop from a pipette with a glass stop-cock, so adjusted as to allow from fifteen to twenty drops to fall in a minute into a mixture of equal volumes of the strongest nitric and sulphuric acids cooled by very cold water.

In repeated experiments I have found that, in spite of the precautions taken to cool the acids, it is impossible to avoid an accident now and then, since, when the action reaches a certain intensity, just as in the oxydation of uric acid or cotton, the experiment ends in an explosion or a violent evolution of nitrous fumes. When such a result occurs in making glonoine, the bystander seldom escapes a severe headache, even though the experiment be conducted in the open air.

After glycerine equal to half the bulk of the mixed acids had dropped in, the whole was thrown into a large volume of cold water, thoroughly washed, drawn off with a pipette, dissolved in ether, and the ethereal solution evaporated on a water-bath. It was in this part of the preparation that the accident occurred which enables me to speak of the consequences which follow the inhalation of the vapor. The glass dish in which the evaporation was being conducted, by some mishap tipped over, spilling half its contents on the hot copper bath, and in a moment the room was full of the mixed vapor of nitroglycerine and ether. Although I stood directly over the water-bath to adjust it, and must have inhaled a large volume of the mixed vapor, no instant bad result followed, but in less than fifteen minutes a headache set in, slight at first, but increasing in intensity by degrees, until in an hour and a half it became almost intolerable. It was accompanied by a good deal of faintness and exhaustion, intolerance of light, and a feeling of great general distress and alarm, in addition to the racking pain. Relief was only obtained at length by the inhalation of a large quantity of ether, the insensibility produced by which was followed by broken and disturbed sleep lasting until the following day, which was marked by weakness, exhaustion, and slight headache. These unpleasant symptoms did not finally disappear for three or four days.

It may be remarked that, during all the time that the severe pain and distress lasted, consciousness was never lost for an instant. In Mr. Field's case,[2] *two drops* of a solution containing only one drop of glonoine to ninety-nine of rectified spirit produced loss of consciousness and other very alarming symptoms of narcotic poisoning.

The effects of glonoine upon different individuals are exceedingly different and contradictory. Two drops of a diluted solution containing only one drop of nitroglycerine in ninety-nine of

[2] Vide Braithwaite ut supra.

alcohol produces alarming symptoms of poisoning in one person, while another swallows two hundred drops of a similar solution with no other ill effects than a slightly "muddled" feeling in the head. I have experienced unpleasant feelings from tasting exceedingly minute quantities of *pure* nitroglycerine, such as headache, buzzing in the ears, with a feeling of nervousness and depression, although the action of the drug does not seem to be nearly so powerful or so rapid as when it is given in the form of alcoholic solution. Pure nitroglycerine is volatile at ordinary temperatures—a fact which was accidentally discovered in drawing off with a mouth pipette some nitroglycerine which had just been washed with water. Headache and the usual symptoms immediately set in, though not a particle of the liquid touched my mouth or tongue.

The following experiment, which shows that some constitutions are susceptible to the action of one-fortieth of a drop of glonoine, was made with a solution of nitroglycerine containing two and one-half drops of the pure substance to ninety-seven and one-half of alcohol. The solution was dropped upon sugar, and the sugar allowed to dissolve on the tongue.

My general health being good, and my pulse being seventy-nine, about two and one-half hours after a full meal, I took *one* drop of the solution. In two minutes my pulse was ninety-four, with dull, throbbing headache; in five minutes the pulse was one hundred, the headache changing from the back to the front of the head; in ten minutes the pulse was down to eighty-eight, and in fourteen minutes back to its normal rate, seventy-nine, although the headache did not wholly pass off for fifteen minutes more. It will be noticed that a quantity of the solution was taken, equal to only one-fortieth of a drop of pure nitroglycerine.

Walpole, Mass., July, 1863.

ART. XX.—*On the Chemical and Mineralogical Relations of Metamorphic Rocks;* by T. STERRY HUNT, F.R.S., of the Geological Survey of Canada.[1]

[Read before the Geological Society of Dublin, March 12, 1863.]

AT a time not very remote in the history of geology, when all crystalline stratified rocks were included under the common designation of primitive, and were supposed to belong to a period anterior to the fossiliferous formations, the lithologist confined his descriptions to the various species of rocks, without reference to their stratigraphical or geological distribution. But,

[1] From the *Dublin Quarterly Journal of Science*, for 1863.

with the progress of geological science, a new problem is presented to his investigation. While paleontology has shown that the fossils of each formation furnish a guide to its age and stratigraphical position, it has been found that sedimentary strata of all ages up to the Tertiary, inclusive, may undergo such changes as to obliterate the direct evidences of organic life; and to give to the sediments the mineralogical characters once assigned to primitive rocks. The question here arises, whether, in the absence of organic remains, or of stratigraphical evidence, there exists any means of determining, even approximately, the geological age of a given series of crystalline stratified rocks;—in other words, whether the chemical conditions, which have presided over the formation of sedimentary rocks, have so far varied, in the course of ages, as to impress upon these rocks marked chemical and mineralogical differences. In the case of unaltered sediments, it would be difficult to arrive at any solution of this question without greatly multiplied analyses; but in the same rocks, when altered, the crystalline minerals which are formed, being definite in their composition, and varying with the chemical constitution of the sediments, may, perhaps, to a certain extent become to the geologist what organic remains are in the unaltered rocks, a guide to the geological age and succession.

It was while engaged in the investigation of metamorphic rocks of various ages in North America, that this problem suggested itself, and I have endeavored from chemical considerations, conjoined with multiplied observations, to attempt its solution. In this *Journal* for 1858, and in the *Quarterly Journal of the Geological Society of London* for 1859, (p. 488), will be found the germ of the ideas on this subject which I shall endeavor to explain in the present paper.

It cannot be doubted that, in the earlier periods of the world's history, chemical forces of certain kinds were much more active than at the present day. Thus, the decomposition of earthy and alkaline silicates, under the combined influence of water and carbonic acid, would be greater when this acid gas was more abundant in the atmosphere, and the temperature probably higher. The larger amounts of alkaline and earthy carbonates then carried to the sea, from the decomposition of these silicates, would furnish a greater amount of calcareous matter to the sediments; and the chemical effects of vegetation, both on the soil and on the atmosphere, must have been greater during the Carboniferous period, for example, than at present. In the spontaneous decomposition of feldspars, which may be described as silicates of alumina, combined with silicates of potash, soda and lime, these latter bases are removed, together with a portion of silica; and there remains, as the final result of the process, a hydrous silicate of alumina, which constitutes kaolin, or clay. This

change is favored by mechanical division; and Daubrée has shown that by prolonged attrition of fragments of granite under water, the softer and readily cleavable feldspar is in great part reduced to an impalpable powder, while the uncleavable grains of quartz are only rounded, and form a readily subsiding sand—the water at the same time dissolving from the feldspar a certain portion of silica, and of alkali. It has been repeatedly observed, where potash and soda feldspar are associated, that the latter is much the more readily decomposed, becoming friable, and finally being reduced to clay, while the orthoclase is unaltered. The result of combined chemical and mechanical agencies acting upon rocks which contain quartz, and orthoclase, and a soda feldspar, such as albite or oligoclase, would thus be a sand, made up chiefly of quartz and potash feldspar, and a finely divided and suspended clay, consisting for the most part of kaolin and of partially decomposed soda feldspar, mingled with some of the smaller particles of orthoclase and of quartz. With this sediment will also be included the oxyd of iron, and the earthy carbonates set free by the sub-aerial decomposition of silicates like pyroxene and the lime (anorthic) feldspars, or formed by the action of the carbonate of soda derived from the latter upon the lime and magnesia salts of sea water. The debris of hornblende and pyroxene will also be found in this finer sediment. This process is evidently the one which must go on in the wearing away of rocks by aqueous agency, and explains the fact that while quartz, or an excess of combined silica, is for the most part wanting in rocks which contain a large proportion of alumina, it is generally abundant in those in which potash feldspar predominates.

So long as this decomposition of alkaliferous silicates is sub-aerial, the silica and alkali are both removed in a soluble form. The process is often, however, submarine, or subterranean, taking place in buried sediments which are mingled with carbonates of lime and magnesia. In such cases the silicate of soda set free, reacts either with these earthy carbonates, or with the corresponding chlorids of the sea water, and forms in either event a soluble soda salt, and an insoluble silicate of lime and magnesia, which takes the place of the removed silicate of soda. The evidence of such a continued reaction between alkaliferous silicates and earthy carbonates is seen in the large amounts of carbonate of soda, with but little silica, which infiltrating waters constantly remove from argillaceous strata; thus giving rise to alkaline springs, and to natron lakes. In these waters it will be found that soda greatly predominates, sometimes almost to the exclusion of potash. This is due not only to the fact that soda feldspars are more readily decomposed than orthoclase, but to the well known power of argillaceous sediments to abstract from water the potash salts which it already holds in solution. Thus,

when a solution of silicate, carbonate, sulphate, or chlorid of potassium is filtered through common earth, the potash is taken up, and replaced by lime, magnesia, or soda, by a double decomposition between the soluble potash salt and the insoluble silicates or carbonates of the latter bases. Soils in like manner remove, from infiltrating waters, ammonia, and phosphoric and silicic acids, the bases which were in combination with these being converted into carbonates. The drainage water of soils, like that of most mineral springs, contains only carbonates, chlorids, and sulphates of lime, magnesia and soda—the ammonia, potash, phosphoric and silicic acids being retained by the soil.

The elements which the earth retains or extracts from waters are precisely those which are removed from it by growing plants. These, by their decomposition under ordinary conditions, yield their mineral matters again to the soil; but, when decay takes place in water, these elements become dissolved, and hence the waters from peat bogs and marshes contain large amounts of potash and of silica in solution, which are carried to the sea, there to be separated—the silica by protophytes, and the potash by algæ, which latter, decaying on the shore, or in the ooze at the bottom, restore the alkali to the earth. The conditions under which the vegetation of the coal formation grew and was preserved, being similar to those of peat, the soils became exhausted of potash, and are seen in the fire-clays of that period.

Another effect of vegetation on sediments is due to the reducing or deoxydizing agency of the organic matters from its decay. These, as is well known, reduce the peroxyd of iron to a soluble protoxyd, and remove it from the soil, to be afterwards deposited in the forms of iron ochre and ores, which by subsequent alteration, become hard, crystalline, and insoluble. Thus, through the agency of vegetation, the iron-oxyd of the sediments is withdrawn from the terrestrial circulation; and it is evident that the proportion of this element diffused in the more recent sediments must be much less than in those of ancient times. The reducing power of organic matter is further shown in the formation of metallic sulphurets; the reduction of sulphates having precipitated in this insoluble form the heavy metals, copper, lead, and zinc, which, with iron, appear to have been in solution in the waters of early times, but are now by this means also abstracted from the circulation, and accumulated in beds and fahlbands, or by a subsequent process have been redissolved and deposited in veins. All analogies lead us to the conclusion that the primeval condition of the metals, and of sulphur, was, like that of carbon, one of oxydation, and that vegetable life has been the sole medium of their reduction.

The source of the carbonates of lime and magnesia in sedimentary strata is two-fold: first, the decomposition of silicates contain-

ing these bases, such as lime (anorthic) feldspar and pyroxene; and, secondly, the action of the alkaline carbonate, formed by the decomposition of feldspars, upon the chlorids of calcium and magnesium originally present in sea-water, which have thus, in the course of ages, been in great part replaced by chlorid of sodium. The clay or aluminous silicate which has been deprived of its alkali is thus a measure of the carbonic acid removed from the air of the carbonates of lime and magnesia precepitated, and of the amount of chlorid of sodium added to the waters of the primeval ocean.

The coarser sediments, in which quartz and orthoclase prevail, are readily permeable to infiltrating waters, which gradually remove from them the soda, lime and magnesia which they contain, and, if organic matters intervene, the oxyd of iron; leaving at last little more than silica, alumina and potash; the elements of granite, trachyte, gneiss, and mica schist. On the other hand, the finer marls and clays, resisting the penetration of water, will retain all their soda, lime, magnesia and oxyd of iron; and, containing an excess of alumina with a small amount of silica, will, by their metamorphism, give rise to basic lime and soda feldspars, and to pyroxene and hornblende—the elements of diorites and dolerites. In this way, the operation of the chemical and mechanical causes which we have traced naturally divides all the crystalline silico-aluminous rocks of the earth's crust into two types. These correspond to the two classes of igneous rocks, distinguished first by Professor Phillips, and subsequently by Durocher and by Bunsen, as derived from two distinct magmas; which these geologists imagine to exist beneath the solid crust, and which the latter denominates the trachytic and pyroxenic types. I have, however, elsewhere endeavored to show that all intrusive or exotic rocks are probably nothing more than altered and displaced sediments, and have thus their source within the lower portions of the stratified crust, not beneath it.

It may be well in this place to make a few observations on the chemical conditions of rock metamorphism. I accept in its widest sense the view of Hutton and of Bouë, that all the crystalline stratified rocks have been produced by the alteration of mechanical and chemical sediments. The conversion of these into definite mineral species has been effected in two ways: first, by molecular changes, that is to say, by crystallization, and a rearrangement of the particles; and, secondly, by chemical reactions between the elements of the sediments. Pseudomorphism, which is the change of one mineral species into another by the introduction, or the elimination, of some element or elements, presupposes metamorphism; since only definite mineral species can be the subjects of this process. To confound meta-

morphism with pseudomorphism, as Bischoff, and others after him, have done, is therefore an error. It may be further remarked, that, although certain pseudomorphic changes may occur in some mineral species, in veins, and near to the surface, the alteration of great masses of silicated rocks by such a process is as yet an unproved hypothesis.

The cases of local metamorphism in proximity to intrusive rocks go far to show, in opposition to the views of certain geologists, that heat has been one of the necessary conditions of the change. The source of this has been generally supposed to be from below; but to the hypothesis of alteration by ascending heat Naumann has objected that the inferior strata in some cases escape change; and that, in descending, a certain plane limits the metamorphism, separating the altered strata above from the unaltered ones beneath: there being no apparent transition between the two. This, taken in connection with the well-known fact that in many cases the intrusion of igneous rocks causes no apparent change in the adjacent unaltered sediments, shows that heat and moisture are not the only conditions of metamorphism. In 1857, I showed by experiments, that, in addition to these conditions, certain chemical reagents might be necessary, and that water, impregnated with alkaline carbonates and silicates, would, at a temperature not above that of 212° F., produce chemical reactions among the elements of many sedimentary rocks, dissolving silica, and generating various silicates.[2] Some months subsequently, Daubrée found that, in the presence of alkaline solutions, at temperatures above 700° F., various silicious minerals, such as quartz, feldspar, and pyroxene, could be made to assume a crystalline form; and that alkaline silicates in solution at this temperature might combine with clay to form felspar and mica.[3] These observations were the complement of my own, and both together showed the agency of heated alkaline waters to be sufficient to effect the metamorphism of sediments by the two modes already mentioned—namely, by molecular changes, and by chemical reactions. Following upon this, Daubrée observed that the thermal alkaline spring of Plombières, with a temperature of 160° F., had, in the course of centuries, given rise to the formation of zeolites, and other crystalline silicated minerals, among the bricks and cement of the old Roman baths. From this he was led to suppose that the metamorphism of great regions might have been effected by hot springs, which, rising along certain lines of dislocation, and thence spreading laterally, might produce alteration in strata

[2] "Proc. Royal Soc. London," May 7, 1857, and "Phil. Mag." (4), xv, 68; also "Am. Jour. Science" [2], xxii, 437, and xxv, 435.

[3] "Comptes Rendus de l'Acad," Nov. 16, 1857; also "Bull. Soc. Geol. France," (2), xv, 103.

near the surface, while those beneath would in some cases escape change.[4] This ingenious hypothesis may serve in some cases to meet the difficulty pointed out by Naumann; but, while it is undoubtedly true in certain instances of local metamorphism, it seems to be utterly inadequate to explain the complete and universal alteration of areas of sedimentary rocks, embracing many hundred thousands of square miles. On the other hand, the study of the origin and distribution of mineral springs shows that alkaline waters (whose action in metamorphism I first pointed out, and whose efficient agency Daubrée has since so well shown) are confined to certain sedimentary deposits, and to definite stratigraphical horizons; above and below which, saline waters wholly different in character are found impregnating the strata. This fact seems to offer a simple solution of the difficulty advanced by Naumann, and a complete explanation of the theory of the metamorphism of deeply buried strata by the agency of ascending heat—which is operative in producing chemical changes only in those strata in which soluble alkaline salts are present.[5]

When the sedimentary strata have been rendered crystalline by metamorphism, their permeability to water, and their alterability, become greatly diminished; and it is only when again broken down by mechanical agencies, to the condition of soils and sediments, that they once more become subject to the chemical changes which have just been described. Hence, the mean composition of the argillaceous sediments of any geological epoch, or, in other words, the proportion between the alkalies and the alumina, will depend not only upon the age of the formation, but upon the number of times which its materials have been broken up, and the periods during which they have remained unmetamorphosed and exposed to the action of infiltra-

[4] It should be remembered that normal or regional metamorphism is in no way dependent upon the proximity of unstratified or igneous rocks, which are rarely present in metamorphic districts. The ophiolites, amphibolites, euphotides, diorites, and granites of such regions, which it has been customary to regard as exotic or intrusive rocks, are in most cases indigenous, and are altered sediments. I have elsewhere shown that the great outbursts of intrusive dolerites, diorites and trachytes, in southeastern Canada, are found, not among the metamorphic rocks, but among the unaltered strata along their margin, or at some distance removed; and I have endeavored to explain this by the consideration that the great volume of overlying sediments, which, by retaining the central heat, aided in the alteration of the strata now exposed by denudation, produced a depression of the earth's surface, and forced out the still lower and softened strata along the lines of fracture which took place in the regions beyond. See my paper "On some Points in American Geology," *American Jour. Science*, [2], xxxi, 414.

[5] See *Report of the Geological Survey of Canada*, 1853-6, pp 479, 480; also *Canadian Naturalist*, vii, 262. For a consideration of the relations of mineral waters to geological formations, see *General Report on the Geology of Canada* (now in press), p. 61; also chap. xix, on "Sedimentary and Metamorphic Rocks," where most of the points touched in the present paper are discussed at greater length.

ting waters. Thus, for example, that portion of the Lower Silurian rocks in Canada which became metamorphosed before the close of the Paleozoic period will have lost less of its soluble bases than the portion of the same age which still remains in the form of unaltered shales and sandstones. Of these, again, such portions as remain undisturbed by folds and dislocations will retain a larger portion of bases than those strata in which such disturbances have favored the formation of mineral springs; which, even now, are active in removing soluble matters from these rocks. The crystalline Lower Silurian rocks in Canada may be compared with those of the older Laurentian series on the one hand, and with the Upper Silurian or Devonian on the other; but, when these are to be compared with the crystalline strata of Secondary or Tertiary age in the Alps, it cannot be determined whether the sediments of which these were formed (and which may be supposed, for illustration, to have been directly derived from Paleozoic strata) existed up to the time of their translation, in a condition similar to that of the altered or of the unaltered Lower Silurian rocks of Canada. The proportion between the alkalies and the alumina in the argillaceous sediments of any given formation is not, therefore, in direct relation to its age, but indicates the extent to which these sediments have been subjected to the influences of water, carbonic acid, and vegetation. If, however, it may be assumed that this action, other things being equal, has, on the whole, been proportionate to the newness of the formation, it is evident that the chemical and mineralogical composition of different systems of rocks must vary with their antiquity, and it now remains to find in their comparative study a guide to their respective ages.

It will be evident that silicious deposits, and chemical precipitates, like the carbonates and silicates of lime and magnesia, may exist with similar characters in the geological formations of any age; not only forming beds apart, but mingled with the impermeable silico-aluminous sediments of mechanical origin. Inasmuch as the chemical agencies giving rise to these compounds were then most active, they may be expected in greatest abundance in the rocks of the earlier periods. In the case of the permeable and more highly silicious class of sediments already noticed, whose chief elements are silica, alumina, and alkalies, the deposits of different ages will be marked chiefly by a progressive diminution in the amount of potash, and the disappearance of the soda which these contain. In the oldest rocks, the proportion of alkali will be nearly or quite sufficient to form orthoclase and albite with the whole of the alumina present; but, as the alkali diminishes, a portion of the alumina will crystallize, on the metamorphism of the sediments, in the form of a potash-mica, muscovite, or margarodite. While the oxygen

ratio between the alumina and the alkali in the feldspar just named is 3 : 1, it becomes 6 : 1 in margarodite, and 12 : 1 in muscovite. The appearance of these micas in a rock, then, denotes a diminution in the amount of alkali, until in some strata the feldspar almost entirely disappears, and the rock becomes a quartzose mica schist. In sediments still farther deprived of alkali, metamorphism gives rise to schists filled with crystals of kyanite, or andalusite, simple silicates of alumina, into whose composition alkalies do not enter; or, in case the sediment still retains oxyd of iron, staurotide and iron-alumina garnet take their places. The matrix of these minerals is generally a quartzose mica schist. The last term in this exhaustive process appears to be represented by the disthene and pyrophyllite rocks, which occur in some regions of crystalline schists.

In the second class of sediments, we have alumina in excess, with a small proportion of silica, and a deficiency of alkali, besides a variable proportion of silicates or carbonates of lime, magnesia, and oxyd of iron. The result of the processes already described will produce a gradual diminution in the amount of alkali, which is chiefly soda. So long as this predominates, the metamorphism of these sediments will give rise to feldspars like oligoclase, labradorite, or scapolite (a dimetric feldspar); but in sediments where lime replaces a great proportion of the soda, there appears a tendency to the production of denser silicates, like lime-alumina garnet and epidote, which replace the soda-lime feldspars. Minerals like the chlorites, and chloritoid are formed when magnesia and iron replace lime. In all these cases, the excess of the silicates of earthy protoxyds over the silicate of alumina is represented in the altered strata by hornblende, pyroxene, olivine, and similar species; which give rise by their admixture with the double aluminous silicates, to diorite, dolerite, diabase, euphotide, eclogite, and similar compound rocks.

In eastern North America, the crystalline strata, so far as yet studied, may be conveniently classed in five groups, corresponding to as many different geological series, four of which will be considered in the present paper.

1. The Laurentian system represents the oldest known rocks of the globe, and is supposed to be the equivalent of the Primitive Gneiss formation of Scandinavia, and that of the Western Islands of Scotland to which also the name of Laurentian is now applied. It has been investigated in Canada along a continuous outcrop from the coast of Labrador to Lake Superior, and also over a considerable area in Northern New York.

2. Associated with this system is a series of strata characterized by a great development of anorthosites, of which the hypersthenite, or opalescent feldspar-rock of Labrador, may be taken as a type. These strata overlie the Laurentian gneiss, and are

regarded as constituting a second and more recent group of crystalline rocks, to which the name of the Labrador series may be provisionally given. From evidence recently obtained, Sir William Logan conceives it probable that this series is unconformable with the older Laurentian system, and is separated from it by a long interval of time.

3. In the third place, there is a great series of crystalline schists, which are in Canada referred to the Quebec group, an inferior part of the Lower Silurian system. They appear to correspond, both lithologically and stratigraphically, with the Schistose group of the Primitive slate formation of Norway, as recognized by Naumann and Keilhau, and to be there represented by the strata in the vicinity of Drontheim, and those of the Dofrefeld. The Huronian series of Canada in like manner appears to correspond to the Quartzose group of the same Primitive Slate formation. It consists of sandstones, imperfect varieties of gneiss, diorites, silicious and feldspathic schists, passing into argillites, with limestones, and great beds of hematite. Though more recent than the Laurentian and Labrador series, these strata are older than the Quebec group; yet, from their position to the westward of the greatest accumulation of sediments, they have been subjected to a less complete metamorphism than the Paleozoic strata of the East. The Huronian series is as yet but imperfectly studied, and for the present will not be further considered.

4. In the fourth place are to be noticed the metamorphosed strata of Upper Silurian and Devonian age, with which may also be included those of the Carboniferous system in eastern New England. This group has as yet been imperfectly studied, but presents interesting peculiarities.

In the oldest of these, the Laurentian system, the first class of aluminous rocks takes the form of granitoid gneiss, which is often coarse grained and porphyritic. Its feldspar is frequently a nearly pure potash-orthoclase, but sometimes contains a considerable proportion of soda. Mica is often almost entirely wanting, and is never abundant in any large mass of this gneiss, although small bands of mica schist are occasionally met with. Argillites, which, from their general predominance of potash and of silica, are related to the first class of sediments, are, so far as known, wanting throughout the Laurentian series; nor is any rock here met with, which can be regarded as derived from the metamorphism of sediments like the argillites of more modern series. Chloritic and chiastolite schists and kyanite are, if not altogether wanting, extremely rare in the Laurentian system. The aluminous sediments of the second class are, however, represented in this system by a diabase made up of dark green pyroxene and bluish labradorite, often associated with a red alumino-ferrous garnet. This latter mineral also sometimes constitutes

small beds, often with quartz, and occasionally with a little pyroxene. These basic aluminous minerals form, however, but an insignificant part of the mass of strata. This system is further remarkable for the small amount of ferruginous matter diffused through the strata, from which the greater part of the iron seems to have been removed, and accumulated in the form of immense beds of hematite and magnetic iron. Beds of pure crystalline plumbago also characterize this series, and are generally found with the limestones. These are here developed to an extent unknown in more recent formations; and are associated with beds of crystalline apatite, which sometimes attain a thickness of several feet. The serpentines of this series, so far as yet studied in Canada, are generally pale colored, and contain an unusual amount of water, a small proportion of oxyd of iron, and neither chrome nor nickel, both of the latter being almost always present in the serpentines of the third series.

The second, or Labrador series is characterized, as already remarked, by the predominance of great beds of anorthosite, composed chiefly of triclinic feldspars, which vary in composition from anorthite to andesine. These feldspars sometimes form mountain masses, almost without any admixture, but at other times include portions of pyroxene, the latter passing into hypersthene. Beds of nearly pure pyroxenite are met with in this series, and others which would be called hyperite and diabase. These anorthosite rocks are often compact, but more frequently granitoid in structure. They are generally grayish, greenish, or bluish in color, and become white on the weathered surfaces. The opalescent labradorite-rock of Labrador is a characteristic variety of these anorthosites, which often contain small portions of red garnet and brown mica, and, more rarely, epidote and a little quartz. They are sometimes slightly calcareous. Magnetic iron and ilmenite are often disseminated in these rocks, and occasionally form masses or beds of considerable size. These anorthosites constitute the predominant part of the Labrador series, so far as yet examined. They are, however, associated with beds of quartzose orthoclase gneiss, which represent the first class of aluminous sediments, and with crystalline limestones; and they will probably be found, when further studied, to offer a complete lithological series. These rocks have been observed in several areas among the Laurentide Mountains, from the coast of Labrador to Lake Huron, and are also met with among the Laurentian rocks of the Adirondack Mountains; of which, according to Emmons, they form the highest summits.

In the third series, which we have referred to the Lower Silurian age, the gneiss is sometimes granitoid, but less markedly so than in the first; and it is much more frequently micaceous, often passing into micaceous schist, a common variety of which contains disseminated a large quantity of chloritoid. Argillites

abound, and, under the influence of metamorphism, sometimes develop crystalline orthoclase. At other times, they are converted into a soft micaceous mineral and form a kind of mica-schist. Chiastolite and staurotide are never met with in the schists of this series, at least in its northern portions, throughout Canada and New England. The anorthosites of the Labrador series are represented by fine grained diorites, in which the feldspar varies from albite to very basic varieties, which are sometimes associated with an aluminous mineral allied to chlorite in composition. Chloritic schists, frequently accompanied by epidote, abound in this series. The great predominance of magnesia in the forms of dolomite, magnesite, steatite and serpentine, is also characteristic of portions of this series. The latter, which forms great beds (ophiolites), is marked by the almost constant presence of small portions of the oxyds of chrome and nickel. These metals are also common in the other magnesian rocks of the series; green chrome-garnets, and chrome-mica occur; and beds of chrome-iron ore are found in the ophiolites of the series. It is also the gold-bearing formation of eastern North America, and contains large quantities of copper ores in interstratified beds resembling those of the Permian schists of Mansfeld and Hesse. In some parts of this series, pure limestones occur, which contain various crystalline minerals common also to the Laurentian limestones, and to those of the fourth series. The only graphite which has been found in the third series is in the form of impure plumbaginous shales.

The metamorphic rocks of the fourth series, as seen in southeastern Canada, are, for the greater part, quartzose and micaceous schists, more or less feldspathic; which, in the neighboring States, become remarkable for a great development of crystals of staurotide and of red garnet. A large amount of argillite occurs in this series; and, when altered, whether locally by the proximity of intrusive rock, or by normal metamorphism, exhibits a micaceous mineral and crystals of andalusite: so that it becomes known as chiastolite slate in its southern extension. Granitoid gneiss is still associated with these crystalline schists. Gold is not confined to the third series, but is also met with in veins cutting the argillites of Upper Silurian age. The crystalline limestones and ophiolites of eastern Massachusetts, which are probably of this series, resemble those of the Laurentian system; and the coal beds in that region are, in some parts, changed into graphite. It is to be remarked that the metamorphic strata of the third and fourth series are contiguous throughout their extent, so far as examined, but are everywhere separated from the Laurentian and Labrador series by a zone of unaltered Paleozoic rocks.

Large masses of intrusive granite occur among the crystalline strata of the fourth series, but are rare or unknown among the

older metamorphic rocks in Canada. The so-called granites of the Laurentian and Lower Silurian appear to be in every case indigenous rocks; that is to say, strata altered *in situ*, and still retaining evidences of stratification. The same thing is true with regard to the ophiolites and the anorthosites of both series; in all of which the general absence of great masses of unstratified rock is especially noticeable. No evidences of the hypothetical granitic substratum are met with in the Laurentian system, although this is, in one district, penetrated by great masses of syenite, orthophyre, and dolerite. Granitic veins, with minerals containing the rarer elements, such as boron, fluorine, lithium, zirconium, and glucinum, are met with alike in the oldest and newest gneiss in North America. These, however, I regard as having formed, like metalliferous veins, by aqueous deposition in fissures in the strata.

The above observations upon the metamorphic strata of a wide region seem to be in conformity with the chemical principles already laid down in this paper; which it remains for geologists to apply to the rocks of other regions, and thus determine whether they are susceptible of a general application. I have found that the blue crystalline labradorite of the Labrador series of Canada is exactly represented by specimens from Scarvig, in Skye; and the ophiolites of Iona resemble those of the Laurentian series in Canada. Many of the rocks of Donegal appear to me lithologically identical with those of the Laurentian period; while the serpentines of Aghadoey, containing chrome and nickel, and the andalusite and kyanite schists of other parts of Donegal cannot be distinguished from those which characterize the altered Paleozoic strata of Canada. It is to be remarked that chrome- and nickel-bearing serpentines are met with in the same geological horizon in Canada and Norway; and that those of the Scottish Highlands, which contain the same elements, belong to the newer gneiss formation; which, according to Sir Roderick Murchison, would be of similar age. The serpentines of Cornwall, the Vosges, Mount Rosa, and many other regions, agree in containing chrome and nickel; which, on the other hand, seem to be absent from the serpentines of the primitive gneiss-formation of Scandinavia. It remains to be determined how far chemical and mineralogical differences, such as those which have been here indicated, are geological constants. Meanwhile, it is greatly to be desired that future chemical and mineralogical investigations of crystalline rocks should be made with this question in view; and that the metamorphic strata of the British Isles, and the more modern ones of southern and central Europe, be studied with reference to the important problem which it has been my endeavor, in the present paper, to lay before the society.

Montreal, Canada, Jan. 25, 1863.

ART. XXI.—*On the Appalachians and Rocky Mountains as Time-boundaries in Geological History;* by JAMES D. DANA.

THE Appalachian mountains, extending from Labrador to Alabama, and the Rocky chain, facing the Pacific from the Arctic to the Isthmus of Darien, are the two great mountain chains of the North American continent. They are the heights which determine its features—one constituting the Atlantic border-chain, the other, the Pacific, and the two forming the limits of the vast interior continental basin. All other lines of heights are small in comparison.

If the elevation of mountains has ever made epochs in geological history, or time-boundaries between its ages, we should look to the elevation of these chains for the profoundest of all divisions in the chronology of the North American continent. And, corresponding with this expectation, these two cases of mountain-raising stand out as boldly between the grander eras of time, as the mountains themselves do geographically between the oceans and the continental interior. The three eras, after the Azoic, recognized by geologists, are the *Paleozoic*, or ancient time, the *Mesozoic*, or medieval time, and the *Cenozoic*, or recent time; the first and second having their intervening limit between the Carboniferous and Reptilian ages, and the second and third, between the Cretaceous period closing the Reptilian age and the Tertiary commencing the age of Mammals.[1] Now, the elevations of the two mountain chains, referred to, date from the limits of these eras. At the first of these limits, or as the closing act in Paleozoic history, the rocks of the Appalachian region were flexed into numerous folds, in part crystallized, and, over a country more than a thousand miles in length, lifted into mountain ranges. And at the *second*, or as the introduction of Cenozoic time, the mass of the Rocky Mountains began to rise above the ocean.

The fact that the formation of the main portion of the Appalachians took place *after* the close of the Carboniferous age is fixed, beyond all question, as the Professors Rogers and others have shown, by the occurrence of the coal-beds of Pennsylvania, Rhode Island and Nova Scotia among the uplifted folded rocks. The coal-beds are part of the material bent and lifted in the grand process of mountain-making, and, of course, must have been laid down before the disturbance began. The evidence has been abundantly presented elsewhere and need not be here repeated.

[1] Prof. Agassiz, in a recent paper in the Atlantic Monthly, places the close of the Paleozoic after the Devonian. In the writer's view, the whole bearing of the science is against any such new arrangement of the Geological ages.

As the uppermost strata of the coal formation, together with the Permian beds, are wanting in Pennsylvania, although occurring in the Mississippi basin, it is probable, as suggested elsewhere by the writer, that the epoch of uplift and disturbance had its commencement even before the Permian period began; and that from this time it continued its progress, reaching its climax when the Carboniferous age had closed.

Again, it is proved, decisively, that the origin of these mountains preceded the Triassic or earliest period of the Reptilian age (or, at least, the closing part of that period) by the position and nature of the Triassic or Triassico-Jurassic beds. For they lie in valleys or depressions that were made in the formation of the Appalachians; they rest unconformably on rocks that were crystallized or consolidated in the course of the Appalachian revolution; and they are largely made of debris from these crystalline rocks. In addition, the species of fossil plants and of Thecodont and Labyrinthodont Reptiles, whose remains or traces occur in the beds, indicate that at least the older part of the formation is Triassic.

With regard to the Rocky Mountains, it is so well known that the mass of the chain was to a large extent under the sea in the Cretaceous period, and has since been raised 5000 to 6000 feet, and that this elevation commenced before the Tertiary period, or Cenozoic time, opened, that a recital in this place of facts bearing on the point is unnecessary.

The importance of the Appalachian revolution as a time-boundary is greatly enhanced by the history of the Paleozoic era preceding it. No raising of mountains is known to have occurred in North America between the Devonian and Silurian ages; and only some limited uplifts and disturbances between the Devonian and Carboniferous. The only elevations of prominent importance during these ages, of which we have evidence, occurred either at the close of the Lower Silurian or earlier. The Green Mountains, one portion of the Appalachians, date their first emergence, probably, from the close of the Lower Silurian. With a few small exceptions, therefore, the long era from the Azoic to the termination of the Carboniferous age was, comparatively, one of prolonged quiet, in which oscillations of level were in progress over continental areas, but no profound and extensive disturbances. These oscillations throughout the Paleozoic, had been, moreover, most profound along the Appalachian region, and the formations in progress had increased there to ten times the thickness acquired in the interior region—the whole directly preparatory for that making of the mountains which was to commence when Paleozoic time should draw to a close.[2]

[2] See the writer's article on American Geological History, this Journal, xxii, 305, 1856.

With no great epochs of revolution to fix limits to the Silurian, and none to give bounds to the Devonian, the heights of the Appalachians loom up majestically as a time-boundary to the Paleozoic.

It is fitting that the raising of one of the two *border-chains* of the continent—the eastern—should thus mark one of the grandest of the transitions in geological history. The transition was as abrupt in the life of the continent and globe as in its formations; for it was the time when its *ancient* features were to a great extent lost:—when *Trilobites*, *Cyathophylloid* and other old styles of *corals*, and the *Sigillariæ* and *Lepidodendra* of the old forests came to an end; when *Brachiopods* lost their preëminence among Mollusks, and *Crinoids* among Radiates, and *heterocercal Ganoids* among Fishes, and the *Lycopodium* tribe and *Calamites* among Acrogens. The transition from the Devonian to the Carboniferous presents no such abrupt change in living tribes. More than 70 species of *coal-plants*, according to Dawson, have already been identified from the Devonian rocks of North America alone —including species of *Ferns*, *Calamites* and *Lepidodendra* among Acrogens, and of *Sigillariæ* and *Conifers* among Gymnosperms; and some of these Devonian species, both of Acrogens and Gymnosperms, occur also, as this author has observed, among the fossil plants of the Carboniferous age.

The Reptiles of the Carboniferous age are the prominent Medieval type begun in Paleozoic time; and these were precursors of the age of Reptiles which was to follow, just as the Jurassic Mammals were precursors of the succeeding age of Mammals. It would be as right to throw the Jurassic and Cretaceous periods (or half of the Reptilian age) into the Age of Mammals, on account of these precursor Mammals, as the Carboniferous age into the Reptilian, because of its Reptiles. In all history, human as well as geological, each age has its beginning, or the initiation of its great characteristic, in the age preceding.

The second of the two grandest transitions in geological history has its appropriate monument in the Rocky Mountains, the *western* border-chain of the continent. The Rocky-Mountain region had been undergoing changes through all previous time, like the Appalachian anterior to its elevation; for ridges of Azoic origin stand on its slopes or upper plateaus—as the Black Hills of Dacotah, and the Laramie range; and others date their origin probably from epochs in the course of the Paleozoic, and from that of the Appalachian revolution at its close:—we say *probably*, because the precise ages of the ranges along the chain have not yet been determined. But there is no doubt that the *mass* of the chain, through a large part of its area, commenced its rise, as has been stated, just before Cenozoic time began. The elevation was not completed at once, but continued in

progress, as the investigations of Hayden have shown, through much of the Tertiary period.

On the eastern border of the continent, only one epoch of profound disturbance *during the progress* of Mesozoic time (or the Reptilian age)—has been distinguished: namely, that when the Triassico-Jurassic formation underwent displacement, and the trap ridges and dykes that are associated with it were formed in Nova Scotia, the Connecticut valley, the Palisade region of New York and New Jersey, Pennsylvania, Virginia and North Carolina. This subordinate epoch of disturbance divides off the period of these Triassico-Jurassic beds from that of the Cretaceous formation.

At the close of the Mesozoic, there was some elevation of the continent on this same *eastern* border, but it was small in amount, compared with that on the *western*.[3]

The destruction of life closing Mesozoic time was as comprehensive and complete in North America, according to present knowledge, as that closing the Paleozoic. Investigation with reference to this point has already extended over so wide a region, that the fact may be regarded as quite well established. The exceptions that we have most reason to look for are those of oceanic fishes; for these species might have escaped the destroying agency (whether of climate and change of level, or the latter alone) which was in action over the continents and along the ocean's shallow borders.[4]

It is, then, evident that in North America the *two* boldest transitions in the course of the Zoic ages correspond with the raising of the mountain chains of the *two* oceanic borders. Thus time and geography are brought into direct parallelism.

Looking now abroad, we find evidence that the fact, here established as regards North America, has the universality of a fundamental truth or principle.

The epoch of the Appalachian revolution was not only a grand epoch in American history, but also in European. For the greatest disturbances over the continent, and the most extensive metamorphic changes, after those which preceded the Upper Silurian, appear to date from the time between the Carboniferous and Triassic periods, either at the beginning or close

[3] The essential conformability of the Cretaceous and Tertiary beds along part of the Atlantic and Gulf borders shows that even the mos tabrupt epochal transitions in geological history are not accompanied everywhere by disturbances of stratification and cases of unconformability. It is hence no objection to closing the Carboniferous age with the Permian, that the Permian beds and the Triassic in some parts of the world are conformable.

[4] Dr. J. Leidy has questioned, in conversation with the writer, whether the teeth of sharks from the American Cretaceous, that are undistinguishable from some of the Tertiary teeth, belong to distinct species or not. The point is not easily settled, since the teeth in these species often afford unsatisfactory specific characters.

of the Permian period. Murchison remarks, concerning the epoch following the Carboniferous, that it was then "that the coal-strata and their antecedent formations were very generally broken up, and thrown by grand upheavals into separate basins, which were fractured by numberless powerful dislocations." The formation of the main part of the Ural chain—the mountains on the *eastern* border of Europe (dividing the Orient into its eastern and western portion)—has been referred to this time.

Again, the epoch of the elevation of the Rocky Mountains was similarly eminent in European history. From the Triassic onward to the middle or later Cretaceous, there had been in Europe only oscillations of level, and relatively small uplifts or disturbances. The elevation of the range of the Côte d'Or and Cévennes in France, and of the Erzgebirge in Saxony, all northeasterly in course, has been referred to the interval between the Jurassic and Cretaceous periods. But when Mesozoic time was drawing to its close, then commenced the elevation of the Alps, Appenines and other heights of this western border of the Orient (for these mountains belong to the *border-region* of the Orient just as the Rocky Mountains do to that of the Occident, and are not as far distant as the latter from the adjoining ocean). The raising of these mountains, like that of the mountains of western America, was completed in the course of the Tertiary period.

Some of the loftiest ranges of Europe, and also of Asia, were lifted to their places after the Eocene had begun—as if the close of the Cretaceous period were less important as a mountain-making epoch than a later era, and as if Mesozoic time, in order to terminate against the grandest mountain elevations, should be continued to the middle or later Eocene. But the transition in kinds of life which accompanied the transition in time from the Cretaceous to the Tertiary shows that the close of the former was, in fact, the prominent epoch of physical change over the globe, notwithstanding the changes of level which subsequently took place. An early step in those changes that were introductory to Cenozoic time appears to have been that which, on both continents, was attended with the most universal effects. Mountains, as is now well known, have not been made by single heavings of the earth's crust, as waves may be thrown up on the ocean, but are results of a slow, long-continued, and often intermitted, action. And, as the Appalachians were in preparation during the Carboniferous age, and probably occupied in their formation the Permian period, so the Rocky Mountains, Alps, and other heights, while initiated long before, finally commenced their grand movements upward as the Reptilian age was terminating, to end them only with the lapse of the Tertiary.

There are thus two specially prominent periods of mountain-making in Europe, as in America, and they are directly con-

nected with the two grand transitions in the life of the world, that of the Paleozoic to the Mesozoic, and that of the Mesozoic to the Cenozoic.[5]

Asia probably affords similar facts. The two opposing mountain chains of most prominence are the Altai on the north, and the Himalayas on the south. Jurassic rocks occur in the Himalayas, on the northern or Thibet side, to a height of from 14,000 to 18,000 feet, according to Strachey, and extend probably through a length of 400 miles. The elevation of the mountains, according to this author, must have taken place in mass, and subsequently to the Jurassic period. The absence of Cretaceous rocks appears to indicate that some slight emergence, at least, existed during the Cretaceous period. With regard to the exact time of the main part of the elevation, the evidence is not yet satisfactory. It is, however, certain that the western portion, in which Cashmeer lies, was still 15,000 feet below its present level in the early Eocene; and the elevation, whenever commenced, was completed throughout the chain, like that of the Alps and Appenines, only after the Tertiary period had begun. Thus the progress was gradual; and it covered the same part of geological time as that of the loftier mountains of America and Europe. As above remarked, the great transition in the life of the globe which took place at the close of the Cretaceous, shows that, notwithstanding this prolonging of the era of elevation, there was a *crisis* in the movement, climate and otherwise, at the close of Mesozoic time. The great physical changes in progress *then* made their profoundest mark on the world's history.

In South America, there is proof, as Darwin has shown, that the Andes were, to a large extent, raised from the ocean after the close of the Mesozoic. The elevation was not completed at once, any more than that of the Rocky Mountains or Alps, but continued afterwards to increase at intervals, while undergoing oscillations, during the subsequent Tertiary period.[6] The Rocky Mountains and Andes were one, apparently, in time of origin, as they are one in position along the American continent.

Is it not then probable, that over *all* the continents the making of the *border-mountains*—the chains which give the land its dominant features, or rather which are its features—corresponds as in America, with the two grandest epochs in the geological past, or, in other words, gives bounds to Paleozoic and Mesozoic time?

[5] The only other epoch or epochs of like eminence indicated in the North American rocks pertain to Azoic time: at its close, and perhaps at distant epochs preceding, there were mountains made and sedimentary strata thousands of feet in thickness folded and crystallized, the latter on a scale not afterwards equalled unless at the close of the Paleozoic or Mesozoic eras.

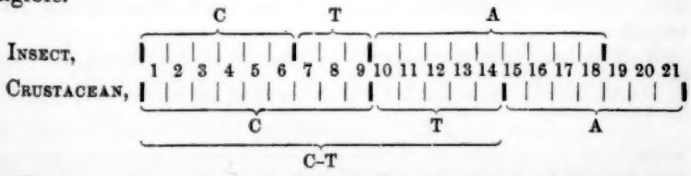
[6] See, on the extensive distribution of Cretaceous or Cretaceo-oolitic beds in the Andes, and on the elevation of these Mountains, Darwin "on South America," pp. 238, 239, and elsewhere; also D. Forbes, Q. J. Geol. Soc. 1861, p. 7.

The uplifting of these mountain regions was produced, as the writer has illustrated elsewhere, by lateral movements in the earth's crust, and mainly in those parts of it that make the bed of the ocean. And as the Atlantic bed stretches from America to Europe, and the Pacific, from America to Asia, there is no violence to reason in supposing that the profound movements which originated the lofty border-chains of one continent should have acted simultaneously (although it may have been very unequally) at the two sides of the oceanic basins, and thus have produced *world-wide* results. If so, we have a universal cause for simultaneous universal effects. There is evidence that in the case of some of the *minor* oscillations there were synchronous parallel movements in the North American and European continents;—as in the formation of marine limestones alike on the two continents in the Subcarboniferous period; in the accumulation of the strata of Millstone grit or coarse sandstone over these limestones; in the slight emergence of the continents and their oscillations below and above the sea-level, during the Carboniferous period, resulting in successive great marshes for coal-making vegetation; and, again, in the simultaneous northern change of level of the Glacial epoch. If distant lands, as these examples prove, moved in sympathy during some of the inferior vibrations of the crust, surely we may look for synchronous action during the raising of the greatest of its mountains. The earth has moved as a unit in all its grander steps of progress.

In view of such facts it is nothing suprising or improbable that the subdivison of time into Paleozoic, Mesozoic and Cenozoic should be registered in the strongest lineaments of the earth's surface.

ART. XXII.—*On the Homologies of the Insectean and Crustacean Types*; by JAMES D. DANA.

IN a note to the article on cephalization, at page 6 of this volume, a brief statement is made by the writer on the relations between the structures of Insects and Crustaceans. The following diagram and explanations will make the subject more intelligible.



The diagram presents to the eye the succession of normal seg-

ments in the two types, that of the Insect or highest Insectean, and that of the Decapod or highest Crustacean (including Crabs, Lobsters, &c.). The spaces between the vertical lines stand for the segments, which are numbered from 1 to 21. C stands for the cephalic portion or head; T, for the thorax; A, for the abdomen; C-T, for the cephalothorax.

The number of normal segments in a Crustacean has been so clearly and conclusively demonstrated by Milne Edwards, that it is unnecessary to add here to what has already been said on the subject. The series and its subdivisions are illustrated in the line above, opposite CRUSTACEAN: *fourteen* segments are shown to belong to the cephalothorax and *seven* to the abdomen. It is established beyond all doubt, that each segment corresponds to a single pair of members, as follows: number 1, to the eyes; 2, 3, to the two pairs of antennæ; then, in the *Decapod*, 4, 5, 6, 7, 8, 9, to organs of the mouth (or mandibles, maxillæ and maxillipeds); 10, 11, 12, 13, 14, to feet; and 15 to 21, to the abdomen.[1]

The abdominal members in all Decapods which have them, and four or more posterior pairs of thoracic members or feet in degradational forms of Decapods (as in Gastrurans or the Squilla group, and in Schizopods), are *two-branched*, or have two jointed terminations proceeding from the second segment: and this is the nearest approach in Decapods to that duplication of the pairs of legs to each segment which occurs in the Iuli and some other related Myriapods.[2]

As the true normal limit of the head in an animal is determined by the fact that this part includes the senses, mouth, and mouth appendages, (for this is demonstrated by the principles of cephalization already explained, if not established on other grounds,) the *head* in the Decapod includes *nine* segments, and the *thorax*, *five*, although there is no constriction of the body to make the division obvious to the eye.

The relation of the Insect-type to the Decapod is at once apparent from a comparison of the two lines in the preceding diagram. Supposing the parallelism rightly presented, the following facts are to be noted.

1. The Insect-type wants the 3 posterior segments of the Crustacean.

2. The head and thorax together of the Insect-type have the same number of segments (nine) as the head alone of the Decapod.

3. The head and thorax of the Insect-type contain *half* of its

[1] In the *Tetradecapod*, 4, 5, 6, 7, pertain to organs of the mouth, and 8, 9, 10, 11, 12, 13, 14, to feet.

[2] The writer has suspected that the multiplication of segments in the Phyllopods might be due to the basal part of each pair of feet becoming a separate body-segment, and that the branches corresponded to the double feet of the Iuli; but as the members in these multiplicative types appear often (if not always) to have the full number of basal joints, this view does not appear to be tenable.

total number of segments (eighteen); the same of the Decapod-type contains *two-thirds* of its total (twenty-one).

4. The head of an Insect contains six segments, which is *one-third* of the total in the Insect-type; that of a Decapod, nine segments, or *three-sevenths* of the total in the Crustacean type. [The head of a Tetradecapod, it may be added, contains seven, or *one-third* the total.]

5. The visceral segments (or those containing the viscera connected with digestion) are the 10th, 11th, 12th, 13th, 14th, in both the Insect-type and the Decapod-type. But in the Insect, the 10th is the first behind the thorax; and in the Crustacean, it is the first behind the head (or the mouth-organs.)[3] The last 2 or 3 normal segments in Insects (that is the 16th, 17th and 18th) are frequently wanting.

In the above homological comparisons, it is assumed that the three anterior normal segments present in a Crustacean are normally and potentially present in an Insect. This will be considered by many as the doubtful point in the above comparisons. But it is proved to be correct by the fact that these three segments are sense-bearing segments in Crustaceans, and the Insect fails in no sense belonging to the Crab. As stated on page 2 of this volume, the absence of a jointed organ is no proof of the absence of the segments, unless it be true, also, that the corresponding sense is wanting.

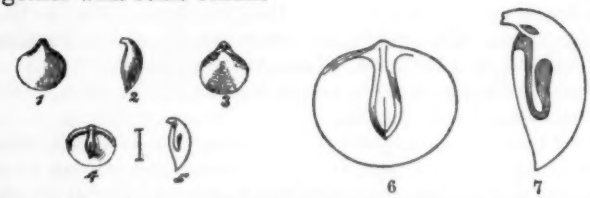
If the constitution of the anterior part of the head in the Insect be still questioned, there is nevertheless good reason for making the mandibular segment in the Articulate type—as it adjoins the centre in embryonic development from which progress goes on forward and backward—normally identical in all groups under that type; and, hence, from this segment, or No. 4 in the Crustacean series, on to No. 18, the parallelism between the Insect and Crustacean must be rightly given; consequently, if there is any doubt, it holds only with regard to Nos. 1, 2 and 3. The law of unity of structure under a type seems, however, to preclude even this chance for doubt.

Comparing the higher Decapods among Crustaceans and the higher Insects, the mean size or mass is about as 50 to 1. This ratio indicates approximately the amount of condensation in the Articulate structure connected with the elevation of grade from the typical Crustacean to the typical Insectean.

[3] Only in a degradational group of Decapods, that of the Gastrurans, do the viscera reach into the abdominal segments, or those following the 14th. The abdomen is very much elongated in these species, the cephalothoracic portion of the body is comparatively small, and the whole structure is lax and low in grade. The species thus stand apart from the Macrurans, as a separate tribe, equivalent with those of Brachyurans and Macrurans; while the Schizopods are only degradational Macrurans. See *this Journal*, [2], xxv, 338. In the fact that the viscera of the Squilloids or Gastrurans are contained in the *abdominal* portion of the animal, this group appears to approach the order of Insects. But this seeming approximation comes, as observed, through degradation, and is analogous to that between a Limulus and an Insect, as explained on page 6 of this volume.

ART. XXIII.—*On the genus Centronella, with remarks on some other genera of Brachiopoda;* by E. BILLINGS.—In a letter addressed to the Editors of this Journal.

Gentlemen:—As I fear that some confusion may arise with regard to the characters of the genus *Centronella*, I shall feel obliged by your permitting me to publish the original figures illustrating it, together with some others.



Figs. 1, 2, 3. Ventral, lateral, and dorsal views of *Centronella glans-fagea.*—Fig. 4. Interior of dorsal valve, showing the loop.—Fig. 5. Longitudinal section, showing the position of the loop in the interior.—Figs. 6, 7. Figures 4 and 5 as copied by Prof. Hall.[1]

The straight line, between figs. 4 and 5, indicates the total length of the ventral valve of the original specimen, and these figures are, therefore, nearly twice the natural size. Although not very perfect, they represent the form and position of the loop, as seen in the original specimens, correctly, with the exception of one part, (the small triangular process mentioned in the description) which is placed too near the beak. It is not shown at all in Professor Hall's copy of the figure. (Compare figures 5 and 7 above.) The specimen was silicified, and imbedded in a piece of limestone, and it was while dissolving it out, by the application of hydrochloric acid, that I first discovered the characters of the genus. The loop was reduced to a mere skeleton and being in a very fragile condition it fell off and was lost the very day my paper was read. I believe it was the first loop of the kind ever seen. The reflected portion of the loop, or the part represented in fig. 5 as being bent upwards towards the beak, was (in the original specimen), a single flat thin plate. The only difference between it and the plate of *Centronella Julia* (as figured by Prof. Winchell) is in the proportional breadth and extension forwards and backwards.[2] This at the most would be only a specific difference, but I have now some evidence that in *C. glans-fagea* the plate, when perfect, is as large as in *C. Julia.* I have lately succeeded in dissolving out four other specimens, showing the loop. Two of these are

[1] For Professor Hall's paper, referred to here and elsewhere in this article, see this Journal, [2], xxxv, 396, and xxxvi, 11.

[2] See Prof. Winchell's figure 2, this Jour., [2], xxxv, 400.

destitute of the vertical plate. The third showed it imperfectly, but before the loop was fully exposed, it (the plate) disappeared altogether. The fourth, of which I shall give a figure below, retains about half the plate, and is important, as it proves that in the original specimen the whole of the organ was not preserved, and that, when entire, there is little difference between it and that of *C. Julia.*

In publishing the figure of this specimen I beg to place alongside of it the important figure furnished by Dr. Rominger for Prof. Hall's paper, and also my original description of the loop. By comparing these it will be seen that the first view taken of the structure of this genus is the correct one.

"GENUS CENTRONELLA *Billings.*[3] Generic characters.—Shells, having the general form of *Terebratula.* Dorsal valve, with a loop consisting of two delicate ribbon-like lamellæ, which extend about one half the length. These lamellæ at first curve gently outwards and then approach each other gradually, until at their lower extremities they meet at an acute angle; then becoming united they are reflected backwards towards the beak in what appears to be a thin flat vertical plate. Near their origin, each bears upon the ventral side a single triangular crural process."

Fig. 8. A specimen showing the loop with part of the plate, enlarged two diameters.

Fig. 9. Dr. Rominger's figure showing the loop with the plate, broken off.

The remainder of the description compares *Centronella* with other genera and need not be repeated here. Prof. Hall says (p. 405 of his article) that Dr. Rominger's figure "is quite different from that given by Mr. Billings for *Centronella glans-fagea*, and shows essentially the same character as *Cryptonella.*" This expression was used by the distinguished author, to induce the idea that my figures indicate one genus and Dr. Rominger's another and a different genus. In reply, I shall only say that any naturalist can see that the agreement between Dr. Rominger's figure (so far as the latter goes) and my description is absolutely perfect. This figure differs from mine in representing a larger specimen with the plate broken off, in being drawn from a different point of view, and in being more artistically drawn; but it does not indicate a loop constructed upon a different generic plan. In studying the works of a naturalist, we should endeavor, earnestly, to arrive at his meaning by examining and comparing both his figures and his descriptions. The truth must be evolved out of the whole, not from a part. Prof. Hall, I regret to say, does not appear to have followed this course, as he makes no comment upon my description, but confines himself altogether

[3] Canadian Naturalist and Geologist, iv, 131, April, 1859.

to the figures which he misinterprets. The only question (in this connection) that can be of any interest to men of science is this.—*Is there in nature a genus of fossil Brachiopods having the general structure assigned to Centronella by me in* 1854? The discoveries of Prof. Winchell, Dr. Rominger and myself prove very clearly that there is, and I think I have a right to point out that Prof. Hall's recently published observations have added nothing but words to these discoveries.

I shall now offer a few remarks on the genus *Cryptonella*. This genus was first published by Prof. Hall in 1861, at which time, it was said to be founded on the species previously described by him under the names of *Terebratula Lincklœni*, *T. lens*, *T. rectirostra* and *T. planirostra*. Another species was said to be known to him, but it was not named, figured nor described. His description was confined almost altogether to the external characters. The muscular impressions were mentioned, but these give no clue to the structure of the internal organs (such as spires or loops). The substance of the description may be thus expressed.

Genus Cryptonella, *Hall.*—Generic Characters.—Shells, having the general form of *Terebratula*. Internal organs unknown.

I think this genus must stand or fall upon the structure of the internal organs of the species above mentioned, upon which it was originally founded by the author. Should it turn out that in their organization they possess nothing generically new, then, the name *Cryptonella* must become obsolete. It cannot be preserved by shifting it to another genus.

It will be observed that the new edition of the genus in Prof. Hall's paper (p. 401) is in fact founded on the loop of *Centronella Julia*. We have no evidence that this species is congeneric with those which were made the typical forms in 1861. There is no connection yet shown between the *Cryptonella* of 1861 and the *Cryptonella* of 1863.

I repeat that the genus *Cryptonella* can be sustained only by showing that the internal organs of the species upon which it was originally founded are different from those of all previously established genera. As these organs have never been seen, all that is known of the genus is expressed in the short description which I have given above in two lines. I do not say that it is not a new genus, but only, that we have as yet no published proof that it is.

The question whether *C. Julia* belongs to the genus *Centronella* is one of some importance, as its solution depends upon certain principles of classification much discussed of late. That our genera are founded on the modifications of the ultimate parts of animals, there can be no doubt; but how great an amount of modification is required to constitute a generic character is a

matter of opinion. I have reason to believe that the internal organs of the fossil Brachiopoda are much more variable than is generally supposed. Some of these variations I shall mention presently. I shall first make some remarks on the punctate structure of the shell. Prof. Hall says (p. 397) that this character afforded him the principal means of distinguishing the typical species of *Cryptonella*, (as described in 1861) from *Meristella Haskinsi*, *M. Barrisii* and *M. Doris*, afterwards placed by me in *Charionella*. Now this statement astonished me not a little, as it was on account of his having described the shell of *M. Haskinsi* and *M. Doris* as being punctate that I was led to regard these two species as congeneric with *T. Lincklæni*, *T. lens*, *T. planirostra* and *T. rectirostra*.[4] And when afterwards he figured the muscular impressions of *C. eximia*, I concluded that all of the species belonged to *Charionella*. I do not now think the punctate structure of the shell a good guide in classification, as it is a character which pervades the Brachiopoda widely and irregularly, without regard to the affinities of the groups of species in which it occurs.

The grounds upon which *C. Julia* is said to be separable generically from *C. glans-fagea* are the following.

1. The species of *Centronella* heretofore described have the "ventral valve highly convex or subangular in the middle, with the dorsal valve flattened or concave in the middle, or with a median depression, and convex at the sides." (Prof. Hall, p. 402.) In *C. Julia* both valves have a "regular lens-like convexity." In answer to this I have only to state that in almost all genera of Brachiopoda, where the species are numerous, similar differences in the form occur. Let us refer to *Waldheimia* the genus which seems to be nearest to *Centronella*. *W. resupinata* has almost exactly the form of *C. glans-fagea*. *W. carinata* has the form of *C. Hecate*. *W. numismalis* has the form of *C. Julia*. *W. Waltoni*, *W. lagenalis*, *W. ornithocephala* and *W. Celtica* are examples of elongate ovate forms like the typical species of Prof. Hall's genus *Cryptonella*. In *Terebratula* proper, numerous examples of similar and even greater differences might be cited.

2. The hinge, socket, and dental plates are also liable to small variations in structure in different species of the same genus. Thus, most species of *Orthis* have a well developed divaricator process in the dorsal valve. But in *O. Electra* and *O. Tritonia* there is not a vestige of this organ to be seen, the umbo being simply hollowed out into a triangular cavity, to the bottom of which the muscles for opening the valve were no doubt attached. In *O. porambonites*, this process appears in a rudimentary form,

[4] See the 13th *Regents' Report*, where, on p. 84, *M. Haskinsi* is described as having the "*Interior substance of the shell fibrous, with an exterior covering which appears to be punctate*," and on p. 85, *M. Doris* as having the "*shell-structure punctate*."

being represented by a narrow thread-like ridge. In others, it is larger, and, in many, strongly developed. In *Leptæna sordida* and *L. decipiens*, there is no divaricator process. In *Strophomena*, all the Lower Silurian species have a wide foramen; in the Middle and Upper Silurian rocks, species make their appearance with a much narrower aperture, and, in the Devonian, we find many with this opening reduced to a mere line, and some with it obsolete altogether. In this same series, we find also a gradual increase in the extent to which the area is striated; it being smooth in the Lower Silurian, partly striated in the Middle and Upper Silurian, and, in the Devonian, sometimes, as in *S. demissa*, ornamented the whole length with transverse lines.

We have here a gradual transition from *S. alternata* to *S. demissa*, in which two species the characters of the hinge and area are so different that they have been placed in different genera.[1] *Orthisina Verneuilii* has large dental plates, but *O. festinata* none at all. *Spirifera Mosquensis* has these plates extending more than half the length of the valve, but *S. mucronata* is destitute of them. Almost precisely the same differences exist between the internal characters of *Terebratula vitrea* and *T. elongata* as those relied upon for the separation of *C. Julia* from *C. glans-fagea*. [Thus (as described by Davidson), in *T. elongata* there are "*well-defined dental or rostral plates, leaving slits in the beak of the casts.*" Compare Prof. Hall's figure 4 of *C. Julia* on page 400, and his remarks on page 399, of his paper.]

[In *T. vitrea*, Davidson says, "*No prominent rostral plates, only a simple thickening of the shell, at the dental projections, which leave no slits in the beak of internal casts.*" Compare Prof. Hall's remarks and figures (p. 403), *C. glans-fagea.*]

Mr. Davidson does not consider this character of generic importance (see his *British Permian Brachiopoda*, p. 7, and the *Carboniferous Brachiopoda*, p. 11).

I think, therefore, that Prof. Winchell has rightly placed *C. Julia* in the genus *Centronella*, and that Prof. Hall's endeavors to separate it from that genus and make it the foundation of his *Cryptonella* will not be successful.

[1] There is one species, *S. rhomboidalis* in the Devonian and Carboniferous rocks, which retains the wide foramen and non-striated area of *S. alternata*. But this is a true Lower Silurian form, which appears to have sprung from the stock of *S. alternata*, and lived on through the Middle Silurian, Upper Silurian, and Devonian without change. It may be regarded as a remarkable instance of Darwin's theory of divergence. In the Lower Silurian period this species had numerous closely allied congeners. But during the interval to the Devonian the genus as a whole became gradually changed, *S. rhomboidalis* alone retaining the original aspect. In this comparison, all the species of *Streptorhynchus* are of course excepted.

Montreal, Canada, July, 1863.

Art. XXIV.—*On the Explosive Force of Gunpowder;* by Prof. F. A. P. Barnard.

Authorities very widely differ as to the degree of strain to which heavy guns are subjected in experimental or in service firing; and still more widely in their estimate of the expansive force which gunpowder would be capable of exerting, could it be exploded in a space incapable of enlargement, which it exactly fills. The magnitude of the differences may be illustrated by the following examples.

In the work published in 1742 by Benjamin Robins, entitled "New Principles of Gunnery," the absolute expansive force of gunpowder exploded within its own bulk, is set down at one thousand atmospheres. This estimate was founded on certain experiments which may briefly be described as follows: First, by actually collecting the gases generated by the combustion of a given weight of powder, Mr. Robins inferred that these gases, reduced to the actual temperature previous to explosion, exceed, under the ordinary atmospheric pressure, the bulk of the powder by which they are produced in the ratio of 244 to 1. In order to ascertain the effect upon elasticity produced by the heat of combustion, he drew out a portion of a musket barrel into a conical form, leaving an orifice at that end of only one-eighth of an inch. The other end being closed, he subjected the apparatus to the highest heat of a furnace, and then immersed the conical end (which he first stopped with an iron plug) in water. After the tube had sufficiently cooled, he removed the plug and allowed the water to enter. The amount of the fluid found in the tube after the complete restoration of the original temperature, compared with the entire capacity of the tube itself, furnished the data for computing the expansion which the air had undergone in the furnace. The relative volumes of the air in the tube before and after expansion, when reduced to a common temperature, were determined, by a series of experiments of this kind, to be as 796 to 194$\frac{1}{8}$. Combining the data, Mr. Robins computed the maximum possible elasticity of the gases generated in the firing of gunpowder, at 999$\frac{1}{8}$ atmospheres; or, in round numbers, at 1000. It is here assumed that the heat of burning gunpowder is no greater than that of an ordinary furnace.

At a later period this subject was investigated by Gay Lussac. According to his determination, the bulk of the gases generated in the combustion exceeds the original bulk of the powder, in the ratio of 450 to 1. He estimates the temperature of combustion at 1000° C.; and computes the resulting elastic pressure at more than 2100 atmospheres.

Dr. Hutton, relying upon the approximate correctness of a formula which he had constructed for computing the velocities of projectiles fired from a gun, and taking as data the velocities actually observed, as ascertained by Robins' pendulum, concluded the maximum pressure to be somewhere between 1700 and 2300, thus substantially agreeing with Gay Lussac. The results of Dr. Gregory are not materially different from this, the maximum pressure being put by him at 2250.

In the year 1797, Count Rumford communicated to the Royal Society of London the results of an elaborate series of experiments upon the force of gunpowder, in which the estimates of pressure had been deduced among other methods from the observed effect of small charges of powder in lifting heavy weights. He puts the greatest force actually observed at about 55,000 atmospheres; but, as the charges filled but a portion of the cavity beneath the weight, he infers that the maximum pressure in a space entirely filled with the powder ought to be as high as 101,000 atmospheres.

The processes and results of Rumford are criticised by Piobert (*Traité d'Artillerie*, Paris, 1847), who regards them as unsatisfactory. According to his own determination, the maximum pressure should be about 7500 atmospheres.

Dr. Young, in his lectures on natural philosophy, quotes Euler, Lombard and D. Bernoulli, as giving for the same pressure the value of 10,000 atmospheres. He himself seems to favor a higher estimate, between 30,000 and 50,000 atmospheres.

In *Nichol's Cyclopedia of the Physical Sciences*, under the article "Gunnery," we find this statement: "Various experiments indicate the expansive force [of gunpowder] to be between 25,000 and 32,000 atmospheres." No authorities are cited.

In vol. xxii, 2nd Series of this Journal, is contained an article on the "Pressure of Fired Gunpowder," by W. E. Woodbridge, M.D., in which are detailed the results of some interesting experiments made by the writer, in connection with Maj. Mordecai, U. S. A., at the Washington Navy Yard, upon the pressure which guns actually endure in firing round shot. These experiments are deserving of study and will be examined hereafter; but they are not to the point immediately before us. Mr. Woodbridge however states that he exploded twenty grains of rifle powder in a cast-steel cylinder capable of enduring a pressure of 6200 atmospheres at the maximum—the powder entirely filling the cylinder, and the explosion being effected without escape of gas —without bursting the cylinder.

Mr. Woodbridge also quotes Gen. Antoni, of the Sardinian army, as authority for the statement that fine military powder fired in a cylinder of half an inch diameter and height, with no opening but the vent through which it is fired, exerts a pressure of 1400 to 1900 atmospheres.

In the *Encyclopedia Britannica*, last edition, article Gun-powder, Mr. Tomlinson, assuming that the gaseous products of the combustion of gunpowder are carbonic oxyd, sulphurous acid and nitrogen exclusively, computes a theoretic enlargement of volume as 1 : 787·3. Assuming further that the elevation of temperature is such as to treple this volume, he make the maximum pressure 2360 atmospheres.

The interesting Reports of Capt. Rodman, upon metals for heavy guns, and upon the qualities of cannon-powder, published in 1861 by authority of the Secretary of War of the United States, contain statements of experiments in which powder was actually exploded in a shell which it could not burst, but which it entirely filled, and of the force actually developed under these circumstances. In these experiments there was an orifice, one-tenth of an inch in diameter, through which the powder was fired, and through which the gases might of course more or less rapidly escape. The highest pressure registered by the gauge employed for the purpose, contrived by Capt. Rodman himself and described in the volume, was 185,000 lbs. per sq. in.—equivalent to more than 12,000 atmospheres. For certain reasons which he mentions, Capt. Rodman thinks that this is below the true pressure, "so that," he concludes, "I should feel perfectly safe in fixing the inferior limit of the pressure per square inch due to the combustion of gunpowder in its own volume, at, in round numbers, 200,000 pounds."

On the other hand, Mr. Norman Wiard, of New York City, a practical gun founder of large experience, who has given a great deal of attention to this subject experimentally, in a recently published essay, expresses a very strong conviction that all the estimates of the maximum possible pressure of gunpowder heretofore made are greatly in excess; and that this maximum pressure cannot exceed 743 atmospheres, or about eleven thousand pounds to the square inch.

These examples are cited not with any intention to exhaust the list of authorities, but simply for the purpose of illustrating the wide differences between them. None of the results presented can be said to rest upon entirely unexceptionable data; and among those which most largely differ, are some which seem to possess almost equal claims to acceptance. In the year 1857, however, there was published in Poggendorf's *Annalen* for November, a paper by Messrs. Bunsen and Schischkoff, of Heidelberg, entitled "*Chemische Theorie des Schiesspulver*," in which this subject is investigated with a thoroughness never before attempted, and the data are presented for determining the maximum force of gunpowder in a form which seems to leave nothing to desire.[1] This force is computed by them to be equal to

[1] This Journal, [2], xxxvi, 106.

4373·6 atmospheres. The objects successively aimed at by these investigators were to ascertain, first, by the most rigorous methods of analysis, the nature of the several products resulting from the combustion of gunpowder, and their relative quantities; secondly, the volume of the gaseous products reduced to 0° C., as compared with the original volume of the powder; thirdly, the volume of the fixed products, both at the temperature of experiment and at that of combustion; and finally, the absolute amount of heat evolved in the combustion, and (considering the capacities for heat of the several substances present, and their respective weights) the actual temperature of the whole mass in the instant in which the combustion is complete.

As it appears, in this investigation, that the fixed products occupy no inconsiderable portion of the space which the powder originally filled, it follows that the conclusions of Robins and Gay Lussac, had they been in other respects exact, would have materially underrated the maximum theoretic pressure; since the gaseous products, in an absolutely closed space, are by so much the more compressed as the cavity is practically diminished by the presence of the portions which are not gaseous.

It is impossible to read the article of Messrs. Bunsen and Schischkoff, without being strongly inclined to believe that their conclusion is as near the truth as it is possible, in an inquiry of so difficult a nature, to arrive. At the same time, one cannot fail to observe, in reading it, that they have furnished the means of testing the correctness of their result, by taking the velocities which projectiles of given weight, fired from guns of given length and calibre, are observed to have acquired at the moment of leaving the gun; and computing, according to recognized principles of physics, the initial pressures which would be necessary to produce such velocities.

In making such a computation, one assumption must be made (at least in the first instance) which is not true; and which, in so far as it is not true, will have the effect to make the computed maximum less than the real maximum pressure. This assumption is, that the powder is completely fired before the projectile begins to move. The same assumption was made by Robins and by Hutton, and it is implicitly involved in all the velocity formulæ which are found in treatises on artillery or on ballistics, at the present time. It being assumed, then, that all the gas which the powder is capable of producing is set free before the ball begins to move, we require to know, in order to determine the velocity it will generate in the projectile, the following particulars, viz: the original bulk of the gas, its initial temperature, its bulk at the atmospheric temperature and pressure, its capacity for heat both at constant pressure and at constant volume, the length of the bore of the gun, the part of this length occupied

by the cartridge, and the weight of the projectile. All these data, except those which relate to the gun and projectile, are furnished by the investigation just cited; and the remainder may be deduced or directly taken from any table showing the initial velocities obtained by experiment, and the dimensions of the guns by means of which they were obtained.

Without such an investigation, no determination of the probable maximum pressure of gunpowder which could be deduced from the observed velocities of projectiles thrown by it could be entitled to any confidence. The rate of diminishing pressure of the mixed gases during expansion depends on the ratio of their capacities for heat at constant pressure and at constant volume: and of this nothing had been previously known. The formula of Hutton and the formulæ in present use, for calculating the initial velocities of cannon balls, are founded on the law of Mariotte for the relation of the pressure of a gaseous body to its density. This law furnishes a curve of pressures in which the ordinates diminish as the bulk increases much less rapidly than the real pressures; and accordingly, for the production of a given effect, it makes the higher pressures too low, to compensate for the excess of the lower.

The United States Ordnance Manual furnishes a variety of examples of the initial velocities observed in firing round shot from smooth bore guns of different calibres. The calculations which follow are founded on a selection from these examples. In order to obtain a formula suitable for the purpose, we suppose a to represent the length of the space, measured along the bore, which the liberated gases fill, provided they are entirely set free before the shot begins to move; x, the variable length, measured in like manner, which they fill at any time after the motion has commenced; F, the initial force by which the shot is urged; v, the velocity acquired, and γ the ratio between the capacities for heat of the gases as taken at constant pressure and at constant volume. This ratio requires to be so often referred to, that it seems to be desirable to have some mode of indicating it without circumlocution. The term *thermo-dynamic index* appears to be sufficiently significant, and is believed not to be preoccupied. It is therefore employed in the following discussion to denote the ratio in question. The conditions of the problem give us immediately the following, which is founded on Poisson's well known law for the pressure of expanding gases:

$$dv=\mathrm{F}\left(\frac{a}{x}\right)^{\gamma}dt. \quad \text{And we have also } dx=vdt.$$

$$\text{Hence } vdv=\mathrm{F}\left(\frac{a}{x}\right)^{\gamma}dx. \quad \text{And } v^2=\frac{2\mathrm{F}a^{\gamma}}{1-\gamma}x^{1-\gamma}+\mathrm{C}.$$

$$\text{But when } x=a,\ v=0\ ;\ \text{and } \mathrm{C}=-\frac{2\mathrm{F}a^{\gamma}}{1-\gamma}a^{1-\gamma}=\frac{2\mathrm{F}a}{\gamma-1}.$$

Whence, finally,

$$v^2=\frac{2Fa}{\gamma-1}-\frac{2Fa\gamma}{(\gamma-1)x^{\gamma-1}}=\frac{2Fa}{\gamma-1}\left(1-\frac{a^{\gamma-1}}{x^{\gamma-1}}\right)=\frac{2Fa}{\gamma-1}\left(\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}\right).$$

In order to find the value of F, as compared with gravity, put p for the initial pressure of the gases in atmospheres, estimated at 14·72 lbs. per square inch, W_s for the weight of the shot, S_s for its specific gravity, and b for its diameter in a fraction of a foot, W_p for the weight of the powder and S_p for its specific gravity, 62·5 lbs. for the weight of a cubic foot of water, g for the force of gravity, represented by a velocity of $32\frac{1}{6}$ feet per second, c for the calibre of the gun in a fraction of a foot, L its length of bore in calibres, l the length of the cartridge, and n for the ratio of the weight of the powder to the weight of the shot, or $n=\frac{W_p}{W_s}$. Then the pressure per square inch of the section through the centre of the shot which gravity would produce is equivalent to $\frac{W_s}{144b^2\times\frac{1}{4}\pi}$.

And we have the proportion

$$\frac{W_s}{144b^2\times\frac{1}{4}\pi}:g::14{\cdot}72p:F=\frac{14{\cdot}72\times144b^2gp\times\frac{1}{4}\pi}{W_s}.$$

For W_s, substitute its equivalent, viz:

$$W_s=62{\cdot}5b^3S_s\times\tfrac{1}{6}\pi,$$

and we obtain the following:

$$F=\frac{14{\cdot}72\times144b^2gp\times\frac{1}{4}\pi}{62{\cdot}5b^3S_s\times\frac{1}{6}\pi}=50{\cdot}872\,\frac{gp}{bS_s}.$$

Inasmuch as the whole mass of the powder (or of the products of its combustion) is moved as well as the ball, it is common to make some allowance for this circumstance by considering the weight of the ball to be effectively increased by a certain fraction of the weight of the charge. The fraction fixed on by Hutton as giving the most consistent results was one-third; and in this he has been generally followed. If we adopt the same value, we must substitute for W_s in the foregoing, $W_s+\frac{1}{3}W_p=W_s+\frac{1}{3}nW_s=W_s(1+\frac{1}{3}n)=W_s\left(\frac{3+n}{3}\right)$. Whence

$$F=152{\cdot}616\,\frac{gp}{(3+n)bS_s}.$$

The value of a is a certain fraction (to be presently determined) of the length (l) of the charge. To find l, we have

$$W_p=c^2\times\tfrac{1}{4}\pi\times62{\cdot}5lS_p.$$

Divide this by the value of W_s before given, and we have

$$\frac{W_p}{W_s}=n=\frac{c^2\times\frac{1}{4}\pi\times62{\cdot}5lS_p}{62{\cdot}5b^3S_s\times\frac{1}{6}\pi}=\frac{3c^2lS_p}{2b^3S_s}.$$

From which we deduce

$$l=\frac{2b^3nS_s}{3c^2S_p}.$$

The specific gravity of cannon powder, compacted as it is in the gun, is 1·039. That of cannon shot is 7. Substituting these numbers,

$$l=4{\cdot}491\,\frac{b^3}{c^2}\,n.$$

And if m represent the fractional part of l occupied by a, we obtain

$$a=4{\cdot}491\,\frac{b^3}{c^2}\,mn.$$

Substituting then, in the equation for velocity, the value of F; and also that of a in the factor without the bracket, there will result,

$$v^2=2\times152{\cdot}616\times4{\cdot}491\,\frac{b^3gmnp}{(\gamma-1)(3+n)bc^2S_s}\cdot\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}$$

$$=8538{\cdot}27\,\frac{b^2}{c^2}\cdot\frac{np}{3+n}\cdot\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}},$$

in which reduction we put $\gamma=1{\cdot}39$, as deduced by Messrs. Bunsen and Schischkoff, and employ for m the value ·52856, which will presently be shown to be just. We have therefore, finally,

$$v=92{\cdot}4029\,\frac{b}{c}\sqrt{\frac{np}{3+n}\cdot\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}}.$$

And

$$p=\frac{v^2}{8538{\cdot}27}\cdot\frac{c^2}{b^2}\cdot\frac{3+n}{n}\cdot\frac{x^{\gamma-1}}{x^{\gamma-1}-a^{\gamma-1}}.$$

The increase of the weight of the shot in the foregoing formula, by one-third of the weight of the powder, first introduced by Hutton, and since generally adopted, is empirical entirely. A more just view of the case would be the following. Supposing the gases to be wholly liberated before motion begins, it is evident that, during motion, the stratum of gas next the projectile will be lower in temperature and less in density than the stratum next the bottom of the bore; since the expansion of the former will be opposed by the inertia of the projectile only, and that of the latter by the inertia of both the projectile and the charge. Considering, however, that the fire is communicated next the bottom of the bore, it is evident that the first effect of expansion will be to throw the powder, of which the combustion is yet incomplete, forward against the projectile. This effect may balance, or more than balance, the former, so that there can be no great error in assuming that the centre of gravity of the charge is always in the middle of its length. Before the explosion, the position of the common centre of

gravity of the charge and the projectile may be found by the proportion,

$$W_s+W_p : W_s :: \tfrac{1}{2}(l+b) : k = \frac{\tfrac{1}{2}W_s(l+b)}{W_s+W_p} = \frac{\tfrac{1}{2}(l+b)}{1+n},$$

k being the distance of the common centre of gravity from the centre of the charge. The entire distance of this common centre from the bottom of the bore (which distance we will represent by q) is therefore

$$q = k+\tfrac{1}{2}l = \frac{l+\frac{1}{2}(b+nl)}{1+n}.$$

If, in any stage of the expansion, we represent the length of the charge by l', and the entire distance from the bottom of the bore of the common centre, by q', we shall in like manner obtain

$$q' = k'+\tfrac{1}{2}l' = \frac{l'+\frac{1}{2}(b+nl')}{1+n}.$$

And the movement of the common centre in the mean time, which is $q'-q$, will be

$$q'-q = \frac{2+n}{2+2n}(l'-l).$$

But $l'=x-a+l$; whence

$$q'-q = \frac{2+n}{2+2n}(x-a); \text{ and } d(q'-q) = \frac{2+n}{2+2n}\,dx.$$

If, therefore, we represent by v' the velocity which will be acquired by the common centre, we shall obtain the equation,

$$v'^2 = \frac{2Fa}{\gamma-1} \cdot \frac{2+n}{2+2n} \cdot \frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}};$$

in which F must have the value, $F=50{\cdot}872\,\frac{gp}{(1+n)bS_s}$, because the mass moved is now the entire weight of the powder added to that of the projectile. Substituting and reducing as before, there results the equation,

$$v'^2 = 2846{\cdot}09\,\frac{b^2}{c^2} \cdot \frac{pn}{1+n} \cdot \frac{2+n}{2+2n} \cdot \frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}.$$

Now the velocities of the common centre and of the projectile will be to each other as the spaces simultaneously passed over by them; and the squares of the velocities will be as the squares of those spaces. The space passed over by the common centre is $q'-q$; and that passed over in the same time by the projectile is $x-a$. Hence

$$(q'-q)^2 \left(=\left(\frac{2+n}{2+2n}\right)^2 (x-a)^2\right) : (x-a)^2 :: v'^2 : v^2 = \frac{(2+2n)^2}{(2+n)^2} v'^2.$$

And

$$v^2 = 2846{\cdot}09\,\frac{b^2}{c^2} \cdot \frac{pn}{1+n} \cdot \frac{2+n}{2+2n} \cdot \frac{(2+2n)^2}{(2+n)^2} \cdot \frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}$$

$$= 5692{\cdot}18\,\frac{b^2}{c^2} \cdot \frac{pn}{2+n} \cdot \frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}.$$

And $$v = 75{\cdot}4465\,\frac{b}{c}\sqrt{\frac{pn}{2+n}\cdot\frac{x^{\gamma-1}-a^{\gamma-1}}{x^{\gamma-1}}}.$$

Also $$p = \frac{v^2}{5692{\cdot}18}\cdot\frac{c^2}{b^2}\cdot\frac{2+n}{n}\cdot\frac{x^{\gamma-1}}{x^{\gamma-1}-a^{\gamma-1}}.$$

If the value of v last found be divided by that obtained previously, the ratio is $\sqrt{\frac{6+2n}{6+3n}}$; which, when the powder is half the weight of the shot, is $=0{\cdot}9661$; showing that the velocity obtained by the method we have just been considering, is not, even with so excessive a charge, three and a half per cent less than that deduced from the empirical allowance of one-third the weight of the charge to the weight of the projectile. That allowance is therefore nearly correct. When n is put $=0$, or the weight of the powder disregarded, the two determinations agree, as they ought.

We will now attend for a moment to the method of deducing the value of p, *a priori*.

The materials employed in the manufacture of gunpowder, are mixed nearly in the proportion of one equivalent of saltpetre, one equivalent of sulphur, and three equivalents of carbon. If, in the combustion, the sulphur and potassium be supposed to combine, we may assume the results to consist of one equivalent of sulphid of potassium, one equivalent of free nitrogen, and three equivalents of carbonic acid. One gramme of powder will thus furnish, at 0° C. of temperature, and $0^{mm}{\cdot}760$ of pressure, 82·50 c. c. of nitrogen and 248·40 c. c. of carbonic acid; in all 330·92 c. c. in volume of gaseous products. Any other *probable* combination of the gaseous elements will not increase this volume. The investigations of Messrs. Bunsen & Schischkoff demonstrate, however, that the sulphid of potassium forms but a small portion of the fixed residuum. The sulphur is to a great extent oxydized, and forms sulphate and sulphite of potassa, and a portion of the carbonic acid unites with the same base. A small portion of the nitrate appears also to be undecomposed. One gramme of powder, accordingly, furnishes but 193·1 cubic centimetres of gas.

The gunpowder employed by these experimenters contained an excess of saltpetre above the theoretic proportion, and a deficiency of carbon. The sulphur was also somewhat deficient, and there was about three and a half per cent of oxygen and hydrogen nearly in the proportions to form water. The gaseous products constituted 0·3138 of the total weight, and the solid residuum 0·6804.

In the gunpowder employed in the American military service, the saltpetre is slightly in excess, and also the carbon, the sulphur being in deficiency. It is probable that the results of its

combustion would vary somewhat from those here detailed, but not to such an extent as very materially to affect the calculations which follow.

In the experiments of the Heidelberg investigators, the gunpowder was burned under the ordinary pressure of the atmosphere. It may be objected that, in the chamber of a gun, other forms of combination of the elements may take place; but, though this is possible, it is hardly supposable that greater power would in such a case be developed. A combustion which should produce a greater volume of gas would probably be attended with a less development of heat; so that as much as would be gained in elastic force by one of these circumstances would be lost by the other.

The specific gravity of the fixed residuum at 18° C. was found to be 2·35. By a method of determination devised by the experimenters, which is not described but in which they have full confidence, the specific gravity at the temperature of combustion is ascertained to be 1·5. The temperature of combustion itself was determined by very careful experiment. The details of the method pursued are interesting, but it is unnecessary to present them here. It was found that the heat developed by burning a given weight of powder would be sufficient to raise the temperature of an equal weight of water 619°·5 C. The specific heat of the mixed products of combustion was found, at constant pressure and at constant volume, by multiplying the specific heat of each ingredient by the amount per cent of it present, and taking the sum of the products. The elevation of temperature produced by the combustion, on supposition of no enlargement of the space occupied by the powder, is then obviously found by dividing 619°·5 C. by the specific heat at constant volume. This specific heat being 0·18547, the elevation of temperature is equal to 3340° C.

The specific heat at constant pressure is at the same time 0·20698.

In order to obtain the thermo-dynamic index of the gaseous portions of the mixture, the sum of the products formed, as just described, by the several capacities at constant pressure of those portions, must be divided by the sum of the products similarly formed, by the capacities at constant volume. These sums are, respectively, 0·07672 and 0·05520. Their quotient is 1·39, which is the value of the thermo-dynamic index.

In order to find the original bulk of the gases, or the magnitude of the space within which they are compressed if liberated without expansion, we consider that the bulk of the powder before combustion will be expressed in cubic centimetres by $\frac{W_p}{S_p}$; a cubic centimetre of pure water at maximum density weighing one gramme, and W_p being expressed in grammes. In like

manner, if W_r be the weight of the fixed residuum, expressed in grammes, the bulk of the residuum will be $\frac{W_r}{S_r}$. The space occupied by the gases will therefore be $\frac{W_p}{S_p} - \frac{W_r}{S_r} = 0{\cdot}50872$ c. c. As, at 0° C. and $0^{mm}{\cdot}760$ of pressure, the same gases occupy 193·1 c. c. per gramme weight of powder, the elastic force due to difference of volume only would be expressed by

$$\frac{193{\cdot}1\ W_p}{\frac{W_p}{S_p} - \frac{W_r}{S_r}} = (\text{if } W_p = 1 \text{ gramme}) \frac{193{\cdot}1}{\frac{1}{1{\cdot}039} - \frac{0{\cdot}6806}{1{\cdot}5}} = \frac{193{\cdot}1}{{\cdot}50872} = 379{\cdot}58.$$

Thus, the pressure in atmospheres would be 379·58, if there were no elevation of temperature produced by the combustion. The original bulk of one gramme of powder being $\frac{1}{1{\cdot}039} = {\cdot}96246$ c. c., and the space originally filled by the gases being 0·50872 c. c., the relative original bulk of gas and powder will be $\frac{{\cdot}50872}{{\cdot}96246} = 0{\cdot}52856$, which is therefore the numerical value of a in the foregoing formulæ, when l is unity.

The volume of the gases having been reduced to zero of temperature, the effect of an elevation by combustion of 3340° C., may be computed by assuming the absolute zero at −274° C., which is the latest determination as given by Rankine. Putting then p for the pressure, we shall have

$$p = 379{\cdot}58\left(1 + \frac{3340}{274}\right) = 5006{\cdot}5 \text{ atmospheres.}$$

This value exceeds that found by the experimenters themselves by 632·9 atmospheres. The difference is owing almost entirely to the difference in the assumed specific gravities of the powder: the experimenters having taken this at ·964, while we have employed the value 1·039. When gunpowder is not shaken down, its specific gravity is always less than 1, and when well shaken, is always more than 1. The U. S. Ordnance Manual (edition of 1850) gives ·929 for the specific gravity of loose powder, and 1·039 for that of powder well shaken down. It is obvious that, in a gun, we must adopt the higher value. There is also a slight difference between the determinations, owing to a difference in the assumed place of the absolute zero. The coefficient of expansion employed by the experimenters is $(1+0{\cdot}00366\,t)$, which corresponds to a zero at −273°·225 C. Adopting their specific gravity with the zero at −274°, the pressure would be 4364, or about ten atmospheres less than the determination of the experimenters.

We are now in condition to apply the formulæ above given, to the computation of the velocities which the initial pressure

just assigned ought to be capable of generating in projectiles of given weight, fired with given charges of powder, from guns of given calibre and length; and also the initial pressures which would be necessary, in similar cases, to produce the velocities actually observed. The examples which follow, twenty-five in number, are taken from the U. S. Ordnance Manual, and exhibit the results actually obtained in experimental firing at the Washington Navy Yard. As the guns used were all smooth-bores, and the projectiles round shot, the observed velocity is corrected for the loss by windage. The formula for this correction which experiment has suggested, is,

$$C = A\frac{c-b}{c},$$

in which C is the correction, c and b have the values assigned them in the foregoing formulæ, and A is a constant determined by observation, and is usually put $=6400$ ft.

The particulars which enter into the calculation for each form of gun are the following:—

Kind of gun.	Calibre in inches.	Windage in inches.	L'gth of bore in calibres	Weight of projectile in pounds.	Charges, in pounds.
6 pdr. field,	3·67	0·09	15·67	6·15	1·25, 1·50, 2·00.
12 pdr. field,	4·62	0·10	16·00	12·3	2, 2·5, 3, 4.
12 pdr. siege,	4·62	0·10	22 38	12·3	2, 3, 4.
12 pdr. 25 cal.,	4·62	0·10	25·00	12 3	2, 3, 4, 5, 6, 7, 8.
24 pdr. siege,	5·82	0·14	18·56	24·25	3, 4. 6, 8.
32 pdr. sea-coast,	6·40	0·15	16·78	32 3	4, 5·33, 8, 10·67.

In the table which succeeds, are given the values of v which result from the formula when x is made equal to $L-l+a$; that is, when it has the value which belongs to it at the moment the shot leaves the muzzle. The columns "approximate values of n" and "No. of volumes expansion," are introduced for convenient comparison. The second consists of the values of $\frac{x}{a}$ at the moment of the expulsion of the shot. These numbers are approximate, like the values of n. In the calculation, the exact values are in all cases employed. The column of pressures contains the computed initial pressures which would be necessary to produce the velocities corrected for windage.

The results presented in the following table are certainly surprising. While anything like a close agreement between computation and observation was hardly to be expected, every reason for anticipating a discrepancy would indicate that the computed velocities should be in excess and not in deficiency; and the computed pressures in deficiency and not in excess. The formula assumes that the gases are fully developed before the shot begins to move. In point of fact we know that the combustion of cannon powder is far from complete even when the shot

Kind of gun.	No.	Approx val. of n.	No. of vols. expansion.	Velocity observed.	Velocity cor. for Windage	Velocity computed.	Dif. obs. and comp. Velocity.	Dif. cor. and comp Velocity.	Pres. in atm. which would produce cor. veloc.
6 pdr. field.	1	$\frac{1}{3}$	34	1439	1596	1368	− 71	−228	6820
	2	$\frac{1}{4}$	28+	1563	1720	1465	− 98	−255	6899
	3	$\frac{1}{5}$	21−	1741	1898	1623	−118	−275	6842
12 pdr. field.	4	$\frac{1}{6}$	43+	1370	1508	1257	−113	−251	7208
	5	$\frac{1}{3}$	34+	1486	1624	1373	−113	−251	7008
	6	$\frac{1}{4}$	29−	1635	1773	1471	−164	−302	7274
	7	$\frac{1}{5}$	21+	1834	1972	1630	−204	−342	7329
12 pdr. siege.	8	$\frac{1}{6}$	61	1378	1516	1280	− 98	−236	7023
	9	$\frac{1}{4}$	40+	1674	1812	1505	−169	−307	7259
	10	$\frac{1}{5}$	30	1906	2044	1674	−232	−370	7461
Long 12 pdr.	11	$\frac{1}{6}$	68	1444	1582	1287	−157	−295	7567
	12	$\frac{1}{4}$	45	1742	1880	1515	−227	−365	7713
	13	$\frac{1}{3}$	34−	1951	2089	1688	−263	−401	7673
	14	$\frac{2}{13}$	27−	2098	2236	1825	−273	−411	7518
	15	$\frac{1}{3}$	22	2239	2377	1937	−302	−440	7539
	16	$\frac{7}{13}$	19−	2300	2438	2030	−270	−408	7222
	17	$\frac{2}{3}$	16+	2324	2462	2107	−217	−355	6833
24 pdr. siege.	18	$\frac{1}{4}$	68−	1240	1394	1127	−113	−267	7665
	19	$\frac{1}{6}$	50+	1440	1594	1270	−170	−324	7892
	20	$\frac{1}{4}$	33+	1723	1877	1489	−234	−388	7959
	21	$\frac{1}{3}$	25	1870	2024	1653	−217	−371	7511
32 pdr. sea-coast.	22	$\frac{1}{4}$	60+	1271	1421	1124	−147	−297	7994
	23	$\frac{1}{6}$	45	1430	1580	1266	−164	−314	7799
	24	$\frac{1}{4}$	30−	1640	1790	1482	−158	−308	7302
	25	$\frac{1}{3}$	22	1780	1930	1642	−138	−288	6913
							Mean of pressures,		7369

leaves the muzzle. A medium sized cannon powder has grains three-tenths of an inch in diameter—the largest sized, from six to nine-tenths. Piobert gives, as the conclusion arrived at after a very long and elaborate series of experiments on the rapidity of combustion of powder in lumps, that the combustion advances at the rate of half an inch per second. His conclusion is, moreover, positive, that neither heat nor pressure affect to any sensible degree this rate. A grain of powder $0{\cdot}^{in.}3$ in diameter would therefore be nearly a third of a second in burning, while Rodman's experiments prove that the shot of a 42 pdr. with 10 lbs. powder is but little more than five thousandths of a second in the gun after the fire takes the cartridge. We may, also, by a very simple process of calculation, show that Rodman's experimental determination cannot be far from correct. The actually observed velocity with which a round shot leaves a 12 pdr. gun of 16 calibres length, when the charge is $\frac{1}{4}$ of the weight of the shot, is 1635 ft. The length of the bore is 6 ft., which is therefore about the space through which the pressure acts on the projectile. If this pressure were *constant*, it would expel the ball in the time which it would take the ball to move *twelve* feet after leaving the gun—that is to say, in $\frac{12}{1635} = \frac{1}{140}$ of a second nearly.

But as the pressures near the muzzle are very far below the mean, those near the breech must be greatly above; or the time must be much less than this calculation gives. Since therefore the powder is not by any means completely burned—not even probably on supposition that Piobert's conclusions in regard to the effect of heat and pressure on rapidity of combustion are erroneous—before the elastic force of the developed gases ceases to act on the projectile, we have reason to suppose that, if our determination of maximum theoretic pressure is correct, the velocities computed by our formula will be much in excess of those obtained in actual experiment.

Again, all the circumstances of the actual experiment of which the calculation takes no account are in favor of excess on the side of the calculation. The weight of the sabot, the envelopes of the cartridge, the large amount of heat absorbed by the metal of the gun, the resistance of the air, &c.—all these particulars, if allowed for, would reduce the computed velocity. It is evident that there is error somewhere—either in the determination of the volume of gas, or in that of the heat developed, or in the assumption that the relation of elasticity to volume is the same under all pressures. What is known of carbonic acid, however, under high condensation at ordinary or at low temperatures, does not encourage the belief that its elasticity can, under any circumstances, increase more rapidly than in the inverse ratio of its bulk (temperature remaining constant), but makes it nearly certain that any change which should occur in this respect would be in the opposite direction.

There is one particular in which it is probable that our formula ought to be corrected. We have employed the thermodynamic index of the expanding gases, 1·39, as determined by the Heidelberg experimenters and by ourselves. In doing this, we have disregarded the fact that the fixed products of combustion, which have, originally, the same high temperature as the gaseous, are intimately mingled with the gases, in a state of minute division—that is, in the form of smoke. A true thermodynamic index for the time being must take account of the capacities for heat of the entire mixture; and will therefore be the quotient of the joint capacity at constant pressure, which is 0·20698, by the joint capacity at constant volume 0·18547. This quotient is 1·116 nearly. By substituting this value for γ instead of the former, we obtain results in which the differences are generally in the right direction, though by no means so great as we should be led to anticipate. These results are exhibited in the following table.

The mean of the computed pressures, in this table, is somewhat below the assumed pressure:—that is to say, the difference is in the right direction, but it is not a sufficient difference. It

Kind of Gun.	No.	Approx. val. of n.	No. of vols Expansion	Velocity observed.	Velocity cor. for Windage.	Velocity computed.	Diff. obs. and comp. velocity.	Diff. cor. and comp. velocity.	Pres. in atm. which would produce cor. veloc.
6 pdr. field.	1	$\frac{1}{3}$	34	1439	1596	1687	248	91	4483
	2	$\frac{1}{4}$	28+	1563	1720	1790	227	70	4621
	3	$\frac{1}{5}$	21–	1741	1898	1954	213	56	4722
12 pdr. field.	4	$\frac{1}{3}$	43+	1370	1508	1563	193	55	4659
	5	$\frac{1}{4}$	34+	1486	1624	1689	203	65	4629
	6	$\frac{1}{5}$	29–	1635	1773	1793	158	20	4896
	7	$\frac{1}{6}$	21+	1834	1972	1958	124	−14	5081
12 pdr. siege.	8	$\frac{1}{3}$	61	1378	1516	1618	240	102	4396
	9	$\frac{1}{4}$	40+	1674	1812	1865	191	53	4724
	10	$\frac{1}{5}$	30	1906	2044	2046	140	2	4999
Long 12 pdr.	11	$\frac{1}{3}$	68	1444	1582	1635	191	53	4688
	12	$\frac{1}{4}$	45	1742	1880	1888	146	8	4965
	13	$\frac{1}{5}$	34–	1951	2089	2073	122	−16	5081
	14	$\frac{5}{17}$	27–	2098	2236	2217	119	−19	5092
	15	$\frac{2}{9}$	22	2239	2377	2331	92	−46	5206
	16	$\frac{7}{17}$	19–	2300	2438	2422	122	−16	5074
	17	$\frac{1}{3}$	16+	2324	2462	2498	174	+36	4863
24 pdr. siege.	18	$\frac{1}{4}$	68–	1240	1394	1431	191	37	4754
	19	$\frac{1}{5}$	50+	1440	1594	1591	151	− 3	5027
	20	$\frac{1}{6}$	33+	1723	1877	1829	106	−48	5274
	21	$\frac{1}{7}$	25	1870	2024	2000	130	−24	5127
32 pdr. sea-coast.	22	$\frac{1}{4}$	60+	1271	1421	1421	150	0	5007
	23	$\frac{1}{5}$	45	1430	1580	1577	147	− 3	5023
	24	$\frac{1}{6}$	30–	1640	1790	1811	171	21	4891
	25	$\frac{1}{7}$	22	1780	1930	1976	196	46	4774
							Mean of pressures,		4882

would produce a mean effect upon velocity equal to $\sqrt{\frac{4882}{5007}}=$ about one-eightieth of the whole. But, if we take the time in the gun at $0^{s}{\cdot}006$, and admit that the conclusions of Piobert in regard to the rapidity of combustion are not absurdly in error, we shall be forced to allow that only a fraction of the possible elastic energy of the generated gases comes into play; and that therefore, the computed velocities ought (*if we have correctly determined the maximum pressure*) are very greatly in excess of the velocities actually observed; and should probably exceed them a number of times.

It is probable that further experiment would prove, that, contrary to Piobert's opinion, the combustion of gunpowder must go on more rapidly in the chamber of a gun, than under the circumstances in which it is most easy directly to observe it. But it is not by any means probable that the rapidity of burning is ever great enough to permit us to discard the consideration of time from among the elements which must enter into this problem. On the contrary, the pressures registered by Rodman's indicator, when rifle powder and large grained cannon powder are successively fired in the same gun and with the same weight of projectile, show that the evolution of gas in the second case is

much less rapid than in the first; and observers who have witnessed the firing of the fifteen inch guns state that the mammoth powder grains are visible in full combustion after leaving the muzzle of the gun.

Without a more extended examination of the subject than it has been found practicable to introduce into the present article, (designedly brief,) we should not perhaps be justified in stating an absolutely definite conclusion in regard to the maximum pressure which gunpowder is capable of producing; but it seems impossible to reconcile the actually observed velocities of projectiles thrown by it, with any supposition which should place this maximum at less than ten thousand atmospheres, or one hundred and fifty thousand pounds per square inch.

If we admit such a maximum, however, we find ourselves, on the other hand, obliged to discredit, to a certain extent, the results of the very able and elaborate investigation of Messrs. Bunsen and Schischkoff; and to suppose that they have underestimated the volume of the generated gases, or the amount of the heat developed. In the specimen of powder examined by them, the charcoal was in unusually small proportion. This may have produced a sensible effect upon their results; by leading to a larger production of sulphuric acid, and a smaller of carbonic. Still, after making every reasonable allowance for this consideration, we find it impossible to draw from their analysis a satisfactory explanation of the mechanical effects which gunpowder actually produces; and it seems exceedingly desirable that their investigation should be repeated.

One observation may here be made in regard to the allowance commonly made in calculation, for the effect upon the velocity of projectiles, of the windage of guns. In the table above, examples 1, 2 and 3 are similar in all respects except windage, to examples 5, 6 and 7. The computed velocities are, therefore, in the parallel cases, as they should be, almost exactly equal. But the actually observed velocities materially differ, No. 5 being greater by 47 than No. 1; No. 6, greater by 72 than No. 2; and No. 7, greater by 93 than No. 3. In the first set of examples, the windage is about $\frac{1}{40}$, and in the second $\frac{1}{48}$. The higher observed velocities correspond to the less windage. And it is observed that the effect on velocity of difference of windage seems to increase with the charge. It is thus rendered experimentally evident, that it is a great error to make the correction for loss of velocity by windage a *constant* for all charges in the same gun. That this is an error was indeed *a priori* probable. It is somewhat remarkable that it should have been so long permitted to stand unquestioned.

Washington, July, 1863.

Art. XXV.—*On Childrenite from Hebron in Maine;* by Geo. J. Brush.

In an article on the occurrence of *amblygonite* at Hebron, published in this Journal, vol. xxxiv, p. 243, 1862, I mentioned that it was sometimes associated with a peculiar compact variety of apatite, containing minute prismatic crystals of a hair-brown mineral. The small amount of this hair-brown mineral, at that time in my possession, prevented me from determining fully its specific characters; but subsequent explorations of the Hebron locality, made by Mr. Oscar D. Allen, have furnished a sufficient quantity of the substance for examination, to lead to the conclusion that the mineral is probably a variety of *childrenite.*

It occurs in minute prismatic crystals, rarely over three lines in length and half a line in breadth and height: a full description of the crystalline form together with a comparison with the Tavistock childrenite is given beyond by Professor Cooke. Its other characters are as follows. Hardness =5. Specific gravity =3·03 (taken on less than half a gramme of fragments of crystals). Color dark hair-brown. Translucent. Lustre vitreous and brilliant. Streak white. Fracture uneven. Many of the crystals are partially decomposed, and converted into a lustreless earthy material. When heated in the closed tube, the unaltered material gives off neutral water, and the ignited residue is magnetic. Before the blowpipe in the forceps it swells up into ramifications and fuses on the edges, giving the flame a pale-green color, indicative of phosphoric acid. Fusibility =4, on v. Kobell's scale. Heated on charcoal becomes magnetic; with soda in the platinum loop gives a strong reaction for manganese; with borax and salt of phosphorus gives reactions for both manganese and iron. Decomposed by chlorhydric acid leaving traces of silica as an insoluble residue. Qualitative analysis proved the mineral to be a hydrous phosphate of iron, alumina and manganese.

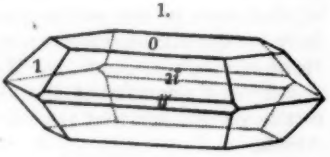
This composition, together with the physical and pyrognostic characters, seem to indicate that the mineral is identical with *childrenite,* a view that is further supported by the crystallographic examination made by Professor Cooke. Only a very few specimens of the Hebron childrenite have as yet been found. This rare species has never before been observed in America.

ART. XXVI.—*Crystallographic examination of the Hebron mineral, and comparison of it with the Childrenite from Tavistock;* by J. P. COOKE, Jr.

THE measurements, which I have made of the crystals from Hebron, sent me by Professor Brush, have afforded the following results.

Form trimetric.—Ratio of axes
$a : b : c = 0{\cdot}977 : 1 : 1{\cdot}482$.

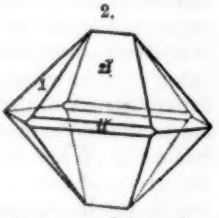
	Observed.	Calculated.
1 on 1 (mac.)	129° 30′*	
1 on 1 (brach.)		101° 36′
1 on 1 (basal)		99° 22′
O on 2$\breve{1}$	127° 10′*	
2$\breve{1}$ on 2$\breve{1}$ (over *O*)	74° 20′	74° 22′



For comparison with the above, I have made the following measurements and figure of the childrenite of Tavistock in Devonshire, England.

Ratio of axes—$a : b : c = 0{\cdot}969 : 1 : 1{\cdot}493$.

	Observed.	Calculated.
1 on 1 (mac.)	130° 10′*	
1 on 1 (brach.)	101° 43′*	
1 on 1 (basal)	98° 44′	98° 40′
2$\breve{1}$ on 2$\breve{1}$ (over *O*)		75° 24′



The values asterisked are in both cases those used in calculating the angles given in the second column. The faces 1 and 2$\breve{1}$, both on the crystals from Hebron and the specimens of childrenite from Tavistock, are so strongly striated parallel to the basal edges that only a few of the angles can in either case be accurately measured. Three of the Hebron crystals and two crystals of the Tavistock childrenite were examined, and the same angles on the different specimens of the same kind were found to agree very closely with the values given above in the column headed 'observed,' the extreme difference not exceeding four or five minutes, which is about the limit of the probable error of the measurements. The angles not given in this column were incapable of exact measurement on the specimen examined, on account of the striation just noticed. Although there is a difference of over half a degree in one of the fundamental angles, yet the general crystallographic characters of the two sets of crystals (the striation and lustre of the different faces) are so nearly the same, that there can be no doubt that the Hebron crystals are a variety of childrenite, differing perhaps from the English childrenite in some not fundamental point of chemical composition.

It will be noticed from the figures (in which the relative proportions of the planes have been preserved as nearly as was possible by the eye) that while on the Tavistock crystals the basal plane is wanting, it is very prominent on the specimens from Hebron, and gives to the crystals from this locality their marked prismatic character. This difference, although from our present mineralogical stand-point unimportant, nevertheless wholly alters the general appearance of the crystal, so that while the Hebron crystals are prismatic, and elongated in the direction of the brachydiagonal, the Tavistock crystals are pyramidal.[1] That differences like this, and even slight differences of angle may be occasioned by the nature of the material in which the mineral crystallized is generally admitted. When, therefore, we consider the peculiar character of the gangue in which the Hebron crystals were found, so different from that at Tavistock, we can hardly be surprised at differences of crystalline form as great as those described above. As already stated, the angles of the Hebron crystals above given are accurate only within four or five minutes, and it is to be hoped that, on further working the locality, more perfect crystals will be found, which will enable us to correct, if necessary, the above measurements.

ART. XXVII.—*Meteoric Iron from Dakota Territory—Description and analysis;* by CHARLES T. JACKSON, M.D., of Boston.

ON the 9th of June last, I received, through Messrs. John W. Shaw & Co. of this city, a mass of meteoric iron from John B. Hoffman, Esq., U. S. Indian Agent for the Ponca tribe of Indians. This mass of metal was supposed by Mr. Hoffman to be some native alloy of silver, and it was sent here to be assayed for that metal.

The mass in my possession weighs ten pounds ten ounces, and is six inches long, five inches wide, and about two inches thick, but is of an irregular form, the weathered or exterior surface being much indented, or wavy and pitted, while its opposite side is columnar, a natural fissure having existed between it and the large mass from which it was detached by the aid of a sledge

[1] At least this is true of all the specimens which have fallen under my observation, but in the original figure of the Tavistock crystals by Brooke, (Brande's *Quarterly Journal*, xvi, 274, and Dana's *Manual of Mineralogy*, ii, 424,) not only the basal plane *O* but also a second set of octahedral planes $\frac{1}{2}$ are represented. This figure in general appearance resembles quite closely the Hebron crystals and is very different from the pyramidal form we have given above. The difference is probably to be explained by the varying characters of the matrix in which the crystals occur at that locality, and it shows that, under certain conditions, the mineral has a tendency to the prismatic form which is only more fully developed in the specimens from Hebron.

hammer. It is stated that this piece was broken from a lump of the same kind, which was estimated to weigh 100 pounds.

It was found on the surface of the ground, in the Dakota Indian territory, ninety miles from any road or dwelling.

Where it has been rubbed and partially polished, the iron has a silvery appearance, and hence the mistake entertained as to its probable nature.

Excepting on the exterior of the columnar portions, which have a steel-like crust, the metal is very soft, and saws or files easily. It has a bright surface when cut. No earthy or stony matter has been found in it, and, judging from its great density, it appears to be solid in its interior. Pieces were sawed off in different places, and these were polished and tested with dilute nitric acid for the production of Widmannstättian figures, but none have thus far been produced, only a scaly like structure, quite fine, is developed by the acid, or when a lump of the iron is dissolved, ridges and fine projecting points are left on the undissolved metal. I noticed the singular phenomenon of the indifferent state of the iron to nitric acid, while dissolving this metal. After a rapid boiling effervescence, with a rush of red fumes of nitrous acid, the chemical action suddenly ceased and could not be renewed by the addition of more nitric acid, nor by a boiling heat, but on inclining the glass beaker, so as to cause the metal to come in contact with the other side of the glass, tumultuous chemical action instantly commenced, and the solution went on rapidly. This seems to show that the electrical state of the metal and of the glass was concerned in the indifferent state of the metal to the acid.

Chemical analysis of the Meteoric Iron.—Qualitative examination soon demonstrated the existence of nickel, phosphorus, tin, cobalt, and chromium in this meteorite.

Its specific gravity, taken with much care, was found to be 7·952. Its hardness that of the softest malleable iron, except on the exterior of the columnar portions, which were as hard as case-hardened iron, resisting the saw and causing a sharp cry under the file. No carbon was found.

The quantitative analysis was effected on two separate pieces, sawed from two of the columns, and the proportion of nickel was twice determined, the iron in both cases being removed as a succinate, by the well known processes.

By blowpipe examination, tin in metallic grains, was obtained, and the presence of small proportions of cobalt and chrome were proved. Phosphoric acid was found by molybdate of ammonia, and was in the analysis separated in the state of pyrophosphate of magnesia.

Although the analysis is not quite complete, yet it is enough so for our present purpose in demonstrating the meteoric nature of this metallic mass under examination.

The following are the per-centage results of my analyses, executed on a gram in each trial.

	1.	2.
Metallic iron	91·735	91·735
" nickel	6·532	7·080
Tin	0·063	0·063
Phosphorus	0·010	0·010
	=98·340	=98·888

The cobalt was proved by the blue color the nickel gave with the borax bead, chrome, as shewn by the bead of the nickel-oxyd in microcosmic salt, the green color being persistent in the reducing flame and coming out as the red color produced by nickel in the hot bead faded. Chlorine was searched for in a solution of 53·7 grs. of the meteorite, but none was discovered.

I have requested Mr. Hoffman to procure and send to me the remainder of this interesting meteorite, and also to inquire of the Indians for other specimens and to procure them if possible. Though of no economical value, these specimens from beyond our world, are of great interest to science, and if our friends on the Pacific shore will look for them, I have no doubt many larger masses of meteoric iron may be found there.

Boston, Aug. 13, 1863.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *On Celestial Dynamics;* by Dr. J. R. Mayer.[1]—[We avail ourselves of a translation by Dr. Debus, of v. Mayer's memoir, now in course of publication in the *London Ed. and Dublin Phil. Mag.*, to lay before our readers this remarkable paper, which has never before been printed in English. In connection with the researches of Joule it certainly marks an era in the history of physical science, and although first published fifteen years ago, the argument remains in all essential points unchanged by later researches. The recent appearance of Dr. Tyndall's most fascinating volume, "*Heat as a mode of Motion*,"[2] has given a degree of general interest to this whole subject which it never would have possessed had not the genius of Tyndall set it forth in a manner equally simple and delightful.—S.]

I. *Introduction.*—Every incandescent and luminous body diminishes in temperature and luminosity in the same degree as it radiates light and heat, and at last, provided its loss be not repaired from some other source of these agencies, becomes cold and non-luminous.

For light, like sound, consists of vibrations which are communicated by the luminous or sounding body to a surrounding medium. It is perfectly clear that a body can only excite such vibrations in another substance when its own particles undergo a similar movement; for there is

[1] *Beiträge zur Dynamik des Himmels, in populärer Darstellung*, von Dr. J. R. Mayer, Stadtarzt in Heilbronn. Heilbronn, 1848. Translated by Dr. H. Debus, F.R.S.

[2] Republished by Appleton, 12mo, pp. 480, New York, 1863.

no cause for undulatory motion when a body is in a state of rest, or in a state of equilibrium with the medium by which it is surrounded. If a bell or a string is to be sounded an external force must be applied; and this is the cause of the sound.

If the vibratory motion of a string could take place without any resistance, it would vibrate for all time; but in this case no sound could be produced, because sound is essentially the propagation of motion; and in the same degree as the string communicates its vibrations to the surrounding and resisting medium its own motion becomes weaker and weaker, until it at last sinks into a state of rest.

The sun has often and appropriately been compared to an incessantly sounding bell. But by what means is the power of this body kept up in undiminished force so as to enable him to send forth his rays into the universe in such a grand and magnificent manner? What are the causes which counteract or prevent his exhaustion, and thus save the planetary system from darkness and deadly cold?

Some endeavored to approach "the grand secret," as Sir Wm. Herschel calls this question, by the assumption that the rays of the sun, being themselves perfectly cold, merely cause the "substance" of heat, supposed to be contained in bodies, to pass from a state of rest into a state of motion, and that in order to send forth such cold rays the sun need not be a hot body, so that, in spite of the infinite development of light, the cooling of the sun was a matter not to be thought of.

It is plain that nothing is gained by such an explanation; for, not to speak of the hypothetical "substance" of heat, assumed to be at one time at rest and at another time in motion, now cold and then hot, it is a well-founded fact that the sun does not radiate a cold phosphorescent light, but a light capable of warming bodies intensely; and to ascribe such rays to a cold body is at once at variance with reason and experience.

Of course such and similar hypotheses could not satisfy the demands of exact science, and I will therefore try to explain in a more satisfactory manner than has been done up to this time the connexion between the sun's radiation and its effects. In doing so, I have to claim the indulgence of scientific men, who are acquainted with the difficulties of my task.

II. *Sources of Heat.*—Before we turn our attention to the special subject of this paper, it will be necessary to consider the means by which light and heat are produced. Heat may be obtained from very different sources. Combustion, fermentation, putrefaction, slaking of lime, the decomposition of chlorid of nitrogen and of gun-cotton, &c., are all of them sources of heat. The electric spark, the voltaic current, friction, percussion, and the vital processes are also accompanied by the evolution of this agent.

A general law of nature, which knows of no exception, is the following:—In order to obtain heat something must be expended; this something, however different it may be in other respects, can always be referred to one of two categories: either it consists of some material expended in a chemical process, or of some sort of mechanical work.

When substances endowed with considerable chemical affinity for each other combine chemically, much heat is developed during the process.

We shall estimate the quantity of heat thus set free by the number of kilogroms of water which it would heat 1° C. The quantity of heat necessary to raise one kilogram of water one degree is called a unit of heat.[3]

It has been established by numerous experiments that the combustion of one kilogram of dry charcoal in oxygen, so as to form carbonic acid, yields 7200 units of heat, which fact may be briefly expressed by saying that charcoal furnishes 7200° of heat.

Superior coal yields 6000°, perfectly dry wood from 3300° to 3900°, sulphur 2700°, and hydrogen 34,600° of heat.

According to experience, the number of units of heat depends only on the quantity of matter which is consumed, and not on the conditions under which the burning takes place. The same amount of heat is given out whether the combustion proceeds slowly or quickly, in atmospheric air or in pure oxygen gas. If in one case a metal be burnt in air and the amount of heat directly measured, and in another instance the same quantity of metal be oxydized in a galvanic battery, the heat being developed in some other place—say, the wire which conducts the current,—in both of these experiments the same quantity of heat will be observed.

The same law also holds good for the production of heat by mechanical means. The amount of heat obtained is only dependent on the quantity of power consumed, and is quite independent of the manner in which this power has been expended. If, therefore, the amount of heat which is produced by certain mechanical work is known, the quantity which will be obtained by any other amount of mechanical work can easily be found by calculation. It is of no consequence whether this work consists in the compression, percusssion, or friction of bodies.

The amount of mechanical work done by a force may be expressed by a weight, and the height to which this weight would be raised by the same force. The mathematical expression for "work done," that is to say, a measure for this work, is obtained by multiplying the height expressed in feet or other units by the number of pounds or kilograms lifted to this height.

We shall take one kilogram as the unit of weight, and one metre as the unit of height, and we thus obtain the weight of one kilogram raised to the height of one metre as a unit measure of mechanical work performed. This measure we shall call a kilogrammetre, and adopt for it the symbol Km.[4]

Mechanical work may likewise be measured by the velocity obtained by a given weight in passing from a state of rest into that of motion. The work done is then expressed by the product obtained by the multiplication of the weight by the square of its velocity. The first method, however, because it is the more convenient, is the one usually adopted; and the numbers obtained therefrom may easily be expressed in other units.

[3] The heat requisite to raise 1 kilogram of water 1° C. will heat 1 lb. av. of water 3·9681° F.

[4] If one metre = 3·2808 English feet, and one kilogram = 2·2045 lbs. av., it follows that one Km = 7·2325 foot-pounds.—Tr.]

The product resulting from the multiplication of the number of units of weight and measures of height, or, as it is called, the product of mass and height, as well as the product of the mass and the square of its velocity, are called "*vis viva* of motion," "mechanical effect," "dynamical effect," "work done," "*quantité de travail*," &c.

The amount of mechanical work necessary for the heating of 1 kilogram of water 1° C. has been determined by experiment to be =367 Km; therefore Km=0·00273 units of heat.[6]

A mass which has fallen through a height of 367 metres possesses a velocity of 84·8 metres in one second; a mass, therefore, moving with this velocity originates 1° C. of heat when its motion is lost by percussion, friction, &c. If the velocity be two or three times as great, 4° or 9° of heat will be developed. Generally speaking, when the velocity is *c* metres, the corresponding development of heat will be expressed by the formula $0{\cdot}000139° \times c^2$.

III. *On the Measure of the Sun's Heat.*—The actinometer is an instrument invented by Sir John Herschel for the purpose of measuring the heating effect produced by the sun's rays. It is essentially a thermometer with a large cylindrical bulb filled with a blue liquid, which is acted upon by the sun's rays, and the expansion of which is measured by a graduated scale.

From observations made with this instrument, Sir John Herschel calculates the amount of heat received from the sun to be sufficient to melt annually, at the surface of the globe, a crust of ice 29·2 metres in thickness.

Pouillet has recently shown by some careful experiments with the lens pyrheliometer, an instrument invented by himself, that every square centimetre of the surface of our globe receives, on an average, in one minute an amount of solar heat which would raise the temperature of one gramme of water 0·4408°. Not much more than one-half of this quantity of heat, however, reaches the solid surface of our globe, since a considerable portion of it is absorbed by our atmosphere. The layer of ice which, according to Pouillit, could be melted by the solar heat which yearly reaches our globe would have a thickness of 30·89 metres.

A square metre of our earth's surface receives, therefore, according to Pouillet's results, which we shall adopt in the following pages, on an average in one minute 4·408 units of heat. The whole surface of the earth is =9,260,500 geographical square miles;[6] consequently the earth receives in one minute 2247 billions of units of heat from the sun.

In order to obtain smaller numbers, we shall call the quantity of heat necessary to raise a cubic mile of water 1° C. in temperature, a cubic mile of heat. Since one cubic mile of water weighs 408·54 billions of kilograms, a cubic mile of heat contains 408·54 billions of units of

[6] How this important result is obtained has been explained in my paper "*Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel.*"

[This essay was published in 1845. At that time de la Roche and Berard's determination of the specific heat of air was generally accepted. If the physical constants used by Mayer be corrected according to the results of more recent investigation, the mechanical equivalent of heat is found to be 771·4 foot-pounds. Mr. Joule finds it =772 foot-pounds.—Tr.]

[6] The geographical mile =7420 metres, and one English mile =1608 metres.

heat. The effect produced by the rays of the sun on the surface of the earth in one minute is therefore, 5·5 cubic miles of heat.

Let us imagine the sun to be surrounded by a hollow sphere whose radius is equal to the mean distance of the earth from the sun, or 20,589,000 geographical miles; the surface of this sphere would be equal to 5326 billions of square miles. The surface obtained by the intersection of this hollow sphere and our globe, or the base of the cone of solar light which reaches our earth, stands to the whole surface of this hollow sphere as $\frac{9,260,500}{4}$: 5326 billions, or as 1 to 2300 millions. This is the ratio of the heat received by our globe to the whole amount of heat sent forth from the sun, which latter in one minute amounts to 12,650 millions of cubic miles of heat.

This amazing radiation ought, unless the loss is by some means made good, to cool considerably even a body of the magnitude of the sun.

If we assume the sun to be endowed with the same capacity for heat as a mass of water of the same volume, and its loss of heat by radiation to affect uniformly its whole mass, the temperature of the sun ought to decrease 1°·8 C. yearly, and for the historic time of 5000 years this loss would consequently amount to 9000° C.

A uniform cooling of the whole of the sun's huge mass cannot, however, take place; on the contrary, if the radiation were to occur at the expense of a given store of heat or radiant power, the sun would become covered in a short space of time with a cold crust, whereby radiation would be brought to an end. Considering the continued activity of the sun through countless centuries, we may assume with mathematical certainty the existence of some compensating influence to make good its enormous loss.

Is this restoring agency a chemical process?

If such were the case, the most favorable assumption would be to suppose the whole mass of the sun to be one lump of coal, the combustion of every kilogram of which produces 6000 units of heat. Then the sun would only be able to sustain for forty-six centuries its present expenditure of light and heat, not to mention the oxygen necessary to keep up such an immense combustion, and other unfavorable circumstances.

The revolution of the sun on his axis has been suggested as the cause of his radiating energy. A closer examination proves this hypothesis also to be untenable.

Rapid rotation, without friction or resistance, cannot in itself alone be regarded as a cause of light and heat, especially as the sun is in no way to be distinguished from the other bodies of our system by velocity of axial rotation. The sun turns on his axis in about twenty-five days, and his diameter is nearly 112 times as great as that of the earth, from which it follows that a point on the solar equator travels but a little more than four times as quickly as a point on the earth's equator. The largest planet of the solar system, whose diameter is about $\frac{1}{10}$th that of the sun, turns on its axis in less than ten hours; a point on its equator resolves about six times quicker than one on the solar equator. The outer ring of Saturn exceeds the sun's equator more than ten times in

velocity of rotation. Nevertheless, no generation of light or heat is observed on our globe, on Jupiter, or on the ring of Saturn.

It might be thought that friction, though undeveloped in the case of the other celestial bodies, might be engendered by the sun's rotation, and that such friction might generate enormous quantities of heat. But, for the production of friction, two bodies, at least, are always necessary which are in immediate contact with one another, and which move with different velocities or in different directions. Friction, moreover, has a tendency to produce equal motion of the two rubbing bodies; and, when this is attained, the generation of heat ceases. If now the sun be the one moving body, where is the other? and if the second body exist, what power prevents it from assuming the same rotary motion as the sun?

But, could even these difficulties be disregarded, a weightier and more formidable obstacle opposes this hypothesis. The known volume and mass of the sun allow us to calculate the *vis viva* which he possesses in consequence of his rotation. Assuming his density to be uniform throughout his mass, and his period of rotation twenty-five days, it is equal to 182,300 quintillions of kilogrammetres (Km). But, for one unit of heat generated, 367 Km are consumed; consequently the whole rotation-effect of the sun could only cover the expenditure of heat for the space of 183 years.——[*To be continued.*]

2. *Kirchhoff's Second Memoir on the Spectrum*[1] has just reached us. The map of the spectrum is continued in this memoir to embrace what was not given in the first, which contained, it will be remembered, the central portions from D to F. In the first plate of the 2d part we have A, B, C to D or 38° to 101°, and in the second plate, from F to part G or 256° to 287°. The work has been executed by Hoffmann, to whom it was entrusted by Kirchhoff, owing to the injury to his eyes in his former researches. Hoffmann has added the lines of numerous elements not before recorded, and gives a table of the atmospheric lines, and their coincidences with the elements and with the lines produced by the electric spark in atmospheric air.

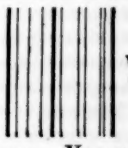
3. *An Improved Spectroscope.—Analysis of the fixed line D;* by Professor Josiah P. Cooke, Jun. (Extracted, by permission, from a letter to Dr. Percy).—I have had a spectroscope constructed, which I believe to be the largest and most powerful ever yet applied to the spectrum. It has nine prisms, filled with CS_2, giving $2\frac{1}{4}$ inches aperture, with telescopes of corresponding size. By means of a conical wheel, against which the backs of the prisms rest, I am able to adjust them with great facility to the angles of least deviation. Two pins on the back of the prism are so adjusted that, when pushed against the wheel, the back of the prism is tangent to the circle. By means of this simple contrivance, I can make the adjustment from one end of the spectrum to the other in a very short time. The prisms are constructed on a plan suggested by my friend Professor Rood. They have wide frames with leveling screws. To the faces, pieces of the best plate glass are cemented, with a mixture of glue and honey. Outside of these, other plates are applied whose outer surfaces have been most carefully

[1] *Untersuchungen über das Sonnenspectrum und die Spectren der chemischen Elemente*, by G. Kirchhoff (2nd Part), with 2 Plates. Berlin, 1863.

ground plain, with castor-oil between. This gives a very perfect prism. As the light is here bent through almost 360°, we have reached about the limit of power, unless we can reflect back the rays over the same path.

This instrument has established the following points:

1st. That the lines of the solar spectrum are as innumerable as the stars of heaven. It shows distinctly at least ten times as many lines as are given by Kirchhoff in his chart, and an infinitude of nebulous bands just on the point of being resolved. To give you an idea I enclose a drawing of the D line of Fraunhofer as seen by it. Kirchhoff gives only three lines—the two broad ones and a faint central one. You notice there are six others and a nebulous band.



2d. It proves that the coincidences between the bright lines of the metallic spectra and the dark lines of the solar spectrum remain perfect, even with this greatly increased power. I am able to spread the two members of the sodium line so far apart that I can readily distinguish $\frac{1}{1000}$ of the intermediate space, and yet the coincidence with the two dark Fraunhofer lines is still absolute.

3d. It shows that many of the bands of the metallic spectra are broad colored spaces, crossed themselves by bright lines. This is the case with the orange band of the strontium spectrum, and with the whole of the calcium and barium spectra to a remarkable extent.—*Chemical News*, July 4, 1863.

4. *Spectrum of Phosphorus—Green coloration of hydrogen by phosphorus.*—Messrs. Christofle and Beilstein, starting from the fact, long ago stated by Wœhler (*Ann. der Chem. und Pharm.*, xxxix, 251), that phosphorous acid communicated a beautiful green color to a hydrogen flame, determine that pure phosphorus, introduced into the hydrogen generation apparatus, produces the same effect. Dusart has also shown the same (*Comptes Rendus*, xliii, 1126), and Blondlot has employed the same facts in toxical examinations for phosphorus (*Jour. de Pharm. et de Ch.*, [3], lv, 25). C. & B. have taken up the inquiry and, by means of the spectrum analysis, have obtained very precise results. This flame, examined by the spectroscope, showed two beautiful green lines on the left of the sodium band, besides a third, less vivid, between the two first and the sodium ray; the same lines are seen by using phosphorous and hypophosphorous acids.

An iron wire, supposed to be quite pure, introduced into the apparatus described, and attacked by HO,SO_3 to avoid any coloration from H Cl, gave a green hydrogen flame, and with the spectrum apparatus gave the same lines as phosphorus. Chemically pure iron (reduced from the oxalate by hydrogen) gave a colorless flame and no green ray in the spectrum.—*Comptes Rendus*, 2 March, 1863. 399.

5. *Osmium Spectrum.*—William Frazer (Dublin) finds that osmium affords three well marked lines of violet-blue color, and a fourth fainter one. These lines, measured by Steinheil's apparatus, occupied 6·0, 6·5, and 7·0 respectively, the fainter line appearing at 5·5, two being placed at the sodium line. It also affords a broad undefined band of blue light to the left of these lines.—*Chem. News, July* 18.

ANALYTICAL CHEMISTRY.

6. *On the Behavior of Dextrin and Gum Arabic toward Albumen.*—RUD. GÜNSBERG (*Sitzungsberichte der Wiener Academie, Mai*, 1862) finds that mineral acids, added to the turbid mixture of fresh white of egg and water, have the same effect as organic acids, if the acid be employed in *small quantity*, viz: the albumen goes into solution more perfectly, instead of being coagulated as happens if the mineral acid (but not the organic acid) be added in large quantity. The solution thus obtained is not coagulated by heating to boiling.

If, to a cold solution of albumen in a dilute mineral or stronger organic acid, a solution of dextrin, prepared from starch, either by help of diastase or sulphuric acid, be added, a heavy precipitate ensues, which shortly settles in form of flocks and is insoluble in excess of dextrin or of acid. The precipitate does not appear to be a simple combination of albumen and dextrin. Günsberg is occupied with its further study. Solution of gum arabic gives a precipitate with acid solution of albumen only when the latter is in large excess, and the precipitate immediately dissolves in excess of the gum. The solution of albumen which of itself is not coagulable by heat, becomes so on the addition of gum arabic. If, therefore, to an acid solution of albumen, gum arabic is added cautiously, a precipitate is formed which disappears on further addition of gum arabic. On heating the mixture, snow-white flocks separate. This observation furnishes a means of distinguishing these two carbo-hydrates. S. W. J.

7. *Detection of nitric acid in waters by means of Brucin.*—KERSTEN (*Ann. Ch. u. Ph.*, cxxv, 224) finds that $\frac{1}{100}$ of a milligramme of nitric acid in a cubic centimetre of water may be plainly detected with help of brucin. The reagents must be specially purified before they can be employed. The water is repeatedly rectified over potash. The brucin is washed with pure water several times to remove nitrates. English sulphuric acid is mixed with 5 per cent of carbonate of ammonia and $\frac{3}{4}$ distilled off in a glass vessel.[1]

Kersten dissolves the brucin in 1000 times its weight of water, pours 1 c. c. of this solution into a champagne glass, adds 1 c. c. of the water to be tested and finally 1 c. c. of sulphuric acid. The latter is allowed to flow down the side of the glass so as to gather beneath the water. At the plane of contact of the two liquids a rose-red zone immediately forms if the nitric acid be present in detectable quantity. S. W. J.

8. *New reaction for Veratrin.*—TRAPP, of St. Petersburg, has observed that the smallest traces of veratrin dissolve in cold concentrated chlorhydric acid, giving a colorless solution, which, on continued boiling, assumes a red color that finally becomes very intense and resembles that of permanganate of potash. This solution remains unaltered by standing for a long time.—*Polytechnisches Notizblatt*, 1863, 96.

9. *Reaction for Molybdenum.*—According to BRAUN, (*Zeitschrift für Analyt. Ch.*, 1863, 36,) sulpho-cyanid of potassium gives, with certain solutions of molybdenum, a red color similar to that produced by the same reagent in solutions of per-salts of iron. The brown solution of Mo_2O_3 in HCl, mixed with concentrated solution of an alkali-sulpho-

[1] According to Goppelsröder (*Verhandl. der naturforsch. Gesells. in Basel.* 1861, 159) *fuming oil-of-vitriol* is free from oxyds of nitrogen. This observation has been confirmed in the Sheffield Laboratory.—[S. W. J.]

cyanid, yields a reddish-yellow liquid which gradually becomes darker and finally appears carmine red.

This reaction is obtained with molybdic acid or solutions of molybdates by putting a fragment of zinc into the liquid, adding a few drops of strong solution of sulpho-cyanid of potassium, and, finally, a little sulphuric or chlorhydric acid, so that a gentle evolution of hydrogen is excited. The red color shortly appears, though it is not permanent.

In this way, $\frac{1}{300000}$ of molybdic acid is recognizable, a quantity less than can be detected by the usual reagents. The sulpho-cyanid of molybdenum is soluble in ether and is taken up by this liquid when agitated with it. It is not dissolved by chloroform or sulphid of carbon.

Since ferric oxyd and hyponitric acid give with alkali-sulpho-cyanids red liquids, they interfere with the direct detection of molybdenum. C. Claus has observed that oxalic and phosphoric acids destroy the sulpho-cyanid of iron, and that in presence of these bodies ferric oxyd is unaltered by the alkali-sulpho-cyanids. Oxalic acid converts the carmine red of sulpho-cyanid ot molybdenum into a reddish-yellow color. Free phosphoric acid has, however, no effect on strong solutions of sulpho-cyanid of molybdenum. The red coloration caused by hyponitric acid in solution of alkali sulpho-cyanids, is destroyed by addition of urea or alcohol as well as by addition of oxalic or phosphoric acid. In order then to recognize molybdenum in presence of ferric oxyd and hyponitric acid, the solution under examination is first boiled with chlorhydric acid to destroy the hyponitric acid, then treated with phosphoric acid and finally with zinc and sulpho-cyanid of potassium. S. W. J.

10. *On the quantitative estimation of Arsenic.*—Wittstein (*Zeitschrift für analytische Chemie*, 1863, 19) observes that the process of drying the ammonio-arseniate of magnesia in vacuo is extremely tedious, while at 100° C. loss of ammonia may occur. He recommends to expel all the water and ammonia and weigh the pyro-arseniate of magnesia. To do this, it is necessary to heat the substance cautiously and gently in a sand-bath, until the ammonia is expelled and the original snow-white color has passed into milk-white. Then the heat is gradually increased until the porcelain crucible almost glows. The residue is $2MgO\,AsO_5$, and no loss of arsenic is to be feared, except through too rapid heating. S. W. J.

TECHNICAL CHEMISTRY.

11. *On the manufacture of Soda, Chlorine, and Sulphuric and Chlorhydric Acids;* by THOMAS MACFARLANE.—In the *Canadian Naturalist* for Feburuary, 1863, Mr. Macfarlane has described a series of processes for the decomposition of sea-salt and the manufacture of soda, chlorine and sulphuric and chlorhydric acids. These new methods, which the inventor has just patented in England, are interesting both in a theoretical and a practical point of view. The starting point in these processes is the fact that when a mixture of dried green vitriol and sea-salt is heated to redness, in a current of air, sesquichlorid of iron is first formed, and then decomposed into peroxyd of iron and chlorine, so that the residue is sulphate of soda and peroxyd of iron. This reaction is facilitated by an admixture of peroxyd of iron, which renders the mass less fusible and keeps it in a porous state. 828 parts of green vitriol are dried and partially peroxydized by a gentle heat, and are then intimately

mixed with 352 parts of sea-salt, and 78 of peroxyd of iron. The whole is then heated to low redness in a muffle calcining furnace, the muffle of which is connected with an exhausting apparatus, by means of which air dried by passing over lime is brought in contact with the mixture. The temperature of this should be kept so low that no perchlorid of iron is sublimed. The mixture is carefully stirred from time to time, and the whole of the chlorine is thus obtained in a gaseous state, mixed with nitrogen, but available for the preparation of bleaching salts and for other purposes. The muffle now contains a mixture of peroxyd of iron and sulphate of soda, for it is claimed that under the above conditions the decomposition of the chlorid of sodium is complete. This mixture is ground with 144 parts of coal and heated to fusion in a reverberatory furnace, the hearth of which is made of ground quick-lime mixed with a little basic slag or glass, and saturated with sulphuret of sodium by means of an admixture of sulphate of soda and coal melted upon its surface. The fused mass after cooling is treated with water, and yields a residue of sulphuret of iron, and a solution of caustic soda colored greenish by a portion of suspended or dissolved sulphuret of iron, which is however precipitated when the carbonic acid from the furnace is passed over the solution, yielding a solution of caustic soda and carbonate of soda which is treated by the ordinary methods.

The residue of proto-sulphuret of iron is washed, and then exposed while moist, on a perforated wooden floor covered with canvass, to the action of the air, by which it is soon converted into sulphate of iron. This is removed by solution from the residue of peroxyd of iron, and we have again the two substances necessary for the decomposition of a new portion of sea-salt.

In this process, the use of sulphuric acid and of peroxyd of manganese are dispensed with, and by the aid of a certain amount of sulphate of iron, which can be employed an indefinite number of times, sea-salt is converted into soda and chlorine, without the employment of any other reagent than a little carbon and the oxygen of the air. The whole of the chlorine is also obtained in an available form, while by the use of peroxyd of manganese one half of this element remains in the state of chlorid. The decomposition of sulphate of soda by peroxyd of iron and charcoal was patented some years since by Blythe and Kopp, but they obtained the sulphate of soda by the action of sulphuric acid on sea-salt, and burned the resulting sulphuret of iron to prepare new portions of the acid.

For the preparation of sulphuric and muriatic acids, Mr. Macfarlane employs, in connection with the chlorine evolved by the method just described, the sulphurous acid obtained by burning sulphur or by calcining iron pyrites. The two gases being mixed in equivalent proportions, and passed with a jet of steam through a condenser filled with coke, yield sulphuric and chlorhydric acids, in accordance with the equation, $SO_2+HO+Cl=SO_3+HCl$. The mixed acids are separated by distillation.

Another process proposed by Mr. Macfarlane for the manufacture of the two acids consists in calcining a mixture of one equivalent each of iron pyrites and sea-salt, with four equivalents of peroxyd of iron. Sulphurous acid is at first evolved, but, in presence of the peroxyd, so

much of this acid is oxydized as to form sulphate of iron sufficient to convert the greater part of the sea-salt into sulphate of soda and chlorine gas, which is thus produced in the second stage of the heating. By charging a series of furnaces with this mixture, and bringing together the chlorine from one and the sulphurous acid from another in the presence of aqueous vapor, a constant production of sulphuric and chlorhydric acids may be kept up, while the soda, with one half of the sulphur of the pyrites, is obtained in the form of sulphate of soda. In a previous paper in the *Canadian Naturalist* for 1862 (page 194), Mr. Macfarlane has detailed numerous experiments made with reference to this transformation into sulphate of soda and chlorine of a mixture of pyrites and sea-salt in presence of a large excess of peroxyd of iron. The use of chlorine for the purpose of converting sulphurous into sulphuric acid was patented by Halmer in 1854, he, however, proposed to employ the chlorine obtained by means of oxyd of manganese, which would probably render his process more expensive than the common one, which depends upon the use of nitrous acid. T. S. H.

PHYSIOLOGICAL CHEMISTRY.

12. *On the excretion of Nitrogen in animals.*—The experiments of Regnault and Reiset, as well as those of Boussingault, have conducted to the assumption that a portion of the nitrogen which a mature animal consumes in its food escapes from the organism in a gaseous form as free nitrogen or ammonia. Bischoff and Voit, in their classical research on the Laws of Nutrition of the Carnivores,[2] are believed to have established, so far as experiments with a single animal, a dog, could serve, that all the nitrogen of food is excreted through the kidneys and intestines and is found again in the urine and fæces.

This result has been confirmed by Henneberg in case of Ruminants, by J. Lehmann in case of swine, and by Joh. Ranke in case of man.

All these experiments were however open to one objection, viz: it was possible that a loss of gaseous nitrogen resulting from a waste of tissue might occur, although the nitrogen of the excreta were equal to that of the food. In such a case the animal must continuously lose flesh.

In order to silence all cavil and to establish so important a law on an irrefutable basis, Voit (*Ann. Chem. u. Ph.*, ii, Sup. Bd., p. 238) has carried out a new series of observations, employing as the subject a pigeon, the animal which according to Boussingault exhales 35 per cent of the nitrogen of its food in a gaseous form. To settle the point in question, it was necessary, Voit remarks, to supply to the animal under trial a definite nourishment for a long period of time. If then, the excreted should be found equal to the ingested nitrogen, any loss of nitrogen by exhalation would result in the wasting away and final death of the animal.

Voit fed a pigeon 124 days (from Oct. 5, 1861, to February 6, 1862) exclusively with peas. The bird consumed 3642·8 grm. air-dry =3132·4 grm. dry (at 100° C.) peas, which contained 4·77 per cent =149·4 grm. of nitrogen. The excrements dried at 100° C. weighed 976 grm. and contained as the mean result of 12 analyses 14·95 per cent =145·9 grm. of nitrogen or 3·5 grm. less than was in the food. The pigeon had

[2] *Die Gesetze der Ernährung des Fleischfressers durch neue Untersuchungen festgestellt.* Leipzig and Heidelberg, 1860.

gradually gained during the trial 70 grm. in weight. Assuming that this increase consisted in nitrogenized tissue, as must be supposed from the character of the food, it corresponds to 2·4 grm. of nitrogen. This added to the quantity found in the excrements leaves but 1·1 grm. unaccounted for, a quantity admitting of a loss of $\frac{9}{10}$ milligramme daily, and so small, considering the duration of the trial, as to be safely attributable to errors of experiment. S. W. J.

II. METALLURGY.

1. NEW WORKS.—*Zusammenstellung der statistischen Ergebnisse des Bergwerks-, Hütten- und Salinen-Betriebes in dem Preussischen Staate während der zehn Jahre von* 1852 *bis* 1861; Bearbeitet von E. ALTHANS, 4to, pp. 156, with 4 lithographic plates. Berlin, 1863.—This valuable collection of statistics of the mineral production of the Prussian States for the ten years, 1852–61, is published as a supplement to the 10th volume of the *Zeitschrift für das Berg- Hütten- und Salinenwesen in dem Preussischen Staate.*

From this we abstract the following in regard to the productions for the year 1861:

Total value of mineral products,	31,234,628	Thalers.
Number of mines worked,	2,304	
Number of workmen employed,	115,341	
Average amount produced by each mine,	13,537	Thalers.
" " " workman,	271	"

During the past twenty-five years the value of the mineral products has increased sixfold, the number of workmen 3½ times, and the number of mines has increased from 1587 to 2304.

We have not space to give further details from this interesting collection, but trust that our government will take some such model as this work for the future reports in regard to the Metallic and Mineral Statistics of the United States; for the absurdities contained in Mr. Kennedy's "*Preliminary Report on the Eighth Census*" (1860), as Prof. J. D. Whitney[1] has pointed out, are such as can only mislead and confuse those who resort to Government documents for information in regard to our metallic and mineral productions. G. J. B.

2. *Handbuch der metallurgischen Hüttenkunde*, von BRUNO KERL. vol. ii, 8vo, pp. 848, with 8 lithographic plates. Freiberg, 1863.—This second volume of the new edition of Kerl's work on Metallurgy treats of the special metallurgy of lead, copper, zinc, cadmium, tin, mercury and bismuth. In the first edition these subjects occupied 384 pages, while in the present volume more than twice the space is covered by them. The completeness and accuracy of Kerl's work commends it not only to practical metallurgists, but also to all scientific men who desire to have a correct idea of those principles in chemistry and metallurgy which find so extended an application in the arts, in the extraction of the useful metals from their ores. No work on metallurgy with which we are acquainted combines so many excellent qualities as this; it is exceedingly well arranged and easy of reference, and conscientiously scrupulous in the citation of authorities and in giving references to original memoirs. The classification of processes, and their illustration by

[1] *Proc. Calif. Acad. Nat. Sci.*, vol. iii, p. 6.

examples, show that the author is not only master of his subject as a practical metallurgist, but that he also has rare skill as a teacher in science. It is a work that should be in every public library, and in the hands of every metallurgist and practical chemist. It is to be completed in four volumes, accompanied with lithographic plates containing drawings of furnaces, etc. G. J. B.

3. *Etat present de la Métallurgie du Fer en Angleterre*, par MM. GRUNER et LAN. 8vo, pp. 850, with nine plates. Paris, 1862.—This work has for the most part been published in a series of memoirs in the *Annales des Mines;* but many who have not the numbers of that valuable Journal will be glad of the opportunity to obtain as a separate book so important a record of the present state of the metallurgy of iron in England. M. Gruner is the Professor of Metallurgy in the Imperial School of Mines at Paris, and M. Lan has a like position in the School for Mines at Saint Etienne. These gentlemen were sent by order of the Minister of Public Affairs in France, in May and June, 1860, to report upon the iron districts of Great Britain; they were offered every facility for making their investigations, and the results which they have published in this volume form a most important contribution to the metallurgy of iron. G. J. B.

4. *Berg- und Hüttenmännisches Jahrbuch der k. k. Bergakademien Leoben und Schemnitz, und der k. k. Montan-Lehranstalt Přibram-Redakteur:* P. TUNNER. 8vo, 261 pp. Wien, 1863.—This annual of the Mining Academies of Leoben and Schemnitz is one of the most important repositories of metallurgical information. The present 12th volume, edited by Director Tunner, is of more than usual interest, as almost half of it is taken up with a report on the objects of interest in metallurgy which were contained in the International Exhibition at London in 1862. G. J. B.

5. *Die Fortschritte des metallurgischen Hüttengewerbes im Jahre*, 1862. Dargestellt von Dr. CARL. FR. ALEX. HARTMANN. Sixth volume, with 3 lithographic plates in folio. 8vo, pp. 352. Leipzig, 1863.—This work is an annual report of the progress of metallurgy, giving a review of all that is published in regard to the metallurgical treatment of the useful metals and new facts in regard to fuel, blowing machines, and furnaces. G. J. B.

6. *International Exhibition of* 1862. *Jurors Report of Class I—Mining, Quarrying, Metallurgy, and Mineral Products.* Reporter, WARRINGTON W. SMYTH, M.A., F.R.S., etc. pp. 44. London, 1862. Price 1 shilling sterling.—This report has but just reached us, and much that it contains has already been republished in this country. The subjects are treated of in the following order:—1. Geological and topographical maps and models, and general collections.—2. Non-metallic mineral substances, coal excepted.—3. Working of mines.—4. Coal and other mineral fuels.—5. Iron.—6. Metals other than iron. Besides being a record of what was contained in Class I, it contains many valuable notes and statistics added by its able reporter, whose position as professor of mining in the Royal School of Mines, and experience as Inspector of Mines to the Crown and to the Duchy of Cornwall, gave him peculiar facilities for obtaining accurate information in regard to these subjects. G. J. B.

III. GEOLOGY.

1. ***Second Annual Report upon the Natural History and Geology of the State of Maine***, 1862. 448 pp. 8vo. 1863.—This volume contains a Report on the Fishes of Maine, by Dr. Ezekiel Holmes; Notes upon certain Mammals of the State, by J. G. Rich; List of Reptiles and Amphibians, by Dr. B. F. Fogg; General observations on Insects, by A. S. Packard, Jr.; Report on the Geology of Maine, by C. H. Hitchcock.

The Geological Report of Prof. Hitchcock occupies 430 pages of the volume. It describes, first, the southern portion of the state, where the rocks are mostly metamorphic, and include limestones as well as granite, syenite, gneiss, mica schist, quartzite and other rocks. Portions of the region are designated Azoic and Taconic; but facts thus far ascertained do not fix the age of the rocks. After presenting much valuable information on this part of the State, it passes to the Schoodic region, or that of the Schoodic Lakes, near the border of New Brunswick, and the northern and unsettled portions of the State. The existence of fossils in New Hampshire near Umbagog Lake is announced; and it is stated that the rock is probably a westward extension of the Oriskany sandstone belt of Maine. Near Moosehead Lake, a mica schist is stated to have afforded a specimen of the *Favosites Gothlandica?* On Farm Island, sandstone occurs containing ripple marks, and the *Fucoides Cauda-Galli.* Descriptions of other fossiliferous regions in this northern portion of the State are given,—the rocks of which are of the age of the Lower Helderberg, Oriskany sandstone, and Cauda-Galli grit—together with lists of fossils.

On p. 402, it is stated that Dr. Dawson has identified 6 more new species of Devonian plants from Perry, Maine; making the whole number of described North American species of terrestrial Devonian plants 75; and in addition, there is material on hand for still increasing this number. The deposits are supposod to "lie between the Chemung and Hamilton groups." It is mentioned also that the Devonian of New Brunswick has afforded Mr. Hartt the wings of insects.

The Report also contains new facts on the Post-tertiary glacial scratches and other phenomena.—We cite the following on Glacial action about Penobscot Bay, from an account at p. 382, by J. De Laski.

"*Ancient Glacial Action in the Southern part of Maine.*

To Mr. George L. Goodale—*Dear Sir:*—I herewith comply with your request to furnish for the ensuing Report of the Scientific Survey of the State of Maine, an account of my examination of the boulder evidence of the Penobscot bay.

From careful personal examination of the surface of the islands and borders of the great fiord of the southern coast of Maine, I have been forced to the conclusion that a glacier once occupied that margin of the state, of a magnitude sufficient to cover the highest hills of the region, and to extend far into the interior towards the north. From a glance at the correct county maps of the locality, we observe that the general trend of the islands, headlands, streams, lakes, harbors, creeks, coves, &c., is north-south, suggesting some law of formation. In these directions, I make no allowance for magnetic variation, which is considerable in the Penobscot bay. There are, indeed, departures from this rule as in the east-west direction of the great thorough-

fare separating the Fox islands, where the natural boundary between the two towns was set up at an infinitely earlier period than that of the boulder age; for the irruption of the trap of North Haven broke through the granite and Taconic slates in a line corresponding to this trend.

We also find the hills not rounded and rough, but having an elongated appearance, and a trend also north-south, as if their sides had been subjected to a gigantic system of sculpturing, on the design that these, too, should be directed towards the south. And furthermore, these hills, even where they attain an elevation of one and two thousand feet, as those of Camden and Mount Desert, present gradual slopes to the north and bold fronts to the south; and, if of granite, they are broken down more or less into step-like precipices of east-west parallels, the debris of which has not been accumulated into taluses, but has been transported south a little distance, often more and more comminuted as we advance.

The formation of the coast is syenitic granite, bordered here and there with a margin of trap or of Taconic slates, highly altered in places, and often converted into cherty flints as on Isle au Haut—and furnishes, from the general barrenness of the surface, a good opportunity to study the boulder phenomena. And this surface is everywhere ridged into furrows, often very deep and in the usual direction of the valleys, &c., and presents the finest examples of embossed rocks as described by Charles H. Hitchcock in his Elements of Geology. This is so remarkably the case that one might, in the foggiest weather, easily point out north, south, &c., by looking at these rocks; for they represent in minature, the hills and mountains of the coast as I have described them. Transverse indentations are everywhere common—*lunoid furrows*, I have called them—from an inch in length to four and five feet, having their horns pointing towards the northeast and northwest, and their steep walls facing the south. *These furrows, in all cases, are sufficient to tell the cardinal points of the compass as one passes along over them.*

Everywhere, too, the boulder striæ may be found on the south sides of these hills at their bases, and on their sides when dipping at large or small angles towards the east or west, in as finely developed examples as are found on their northern slopes. It is a fact beyond controversy, that the boulder phenomena in the Penobscot bay are *sui generis* in character, and owe their existence to one agent and the same period.

I have found these boulder striæ four hundred feet high on the side of Isle au Haut hill—which is five hundred feet above the sea—and on the southern brow of Megunticook, overlooking a precipice of two or three hundred feet, and twelve hundred feet above Camden harbor. Mount Battie, south of that mountain, the nearest to the village of any of those hills, and composed of quartz-oze conglomerate, is everywhere scored and scratched, and has a very abrupt southern face. Vast masses of rock have been torn from it in this direction, and lie around its base. One large boulder here, about forty feet long, must weigh not less than six hundred tons.

There is a series of terraces in Vinalhaven, as you remember, seven hundred yards long, rising one above another, the last wall of which forms the highest margin of a dell running nearly due north-south unbroken for four hundred yards, and from twenty to thirty feet deep, and fifty yards wide. This is a trough cut out of the solid granite—a gigantic and splendid specimen of Nature's sculpturing with her rude stone chisels—all she needed in those days, when she had a vast duration of time before her to prepare a barren country with fruitful soils for the prospective worker, man. Towards the northern extremity of this rim, which is one hundred and fifty feet above the sea, there stands a high rock overlooking the village, apparently in its native bed, presenting a vertical wall towards the south twenty feet high above the soil, and twenty-four broad. No blasting by art, however carefully conducted, could perform a better operation. If this rock be a boulder, as you and I doubted, it must

weigh upwards of a thousand tons. But many thousand tons from the south of it are utterly removed. Going a little further north, we reach one of the highest hills in the town, of granite, two hundred and fifty feet. To the north, we look away down upon a tide "river," now a mile long; but once three, before the land obtained its present height; and earlier still, very much longer. Looking around towards the east and south, we glance over a spacious salt meadow, a densely wooded valley, and a large salt-water pond. This depression must have been cut out of a comparatively level crust. From continued examination of the subject during the last few years, I have seen nothing to induce me to believe that the granite had been materially changed from a horizontal position before the boulder period, as those north–south depressions might suggest. But what was really the depth of the denundation, can only be vaguely conjectured; but I have no doubt that it has been many hundred feet.

The island of Mt. Desert exhibits the boulder phenomena in a more wonderful degree than those places I have mentioned. I presume you have thoroughly explored the locality. You see the southern brows of those lofty granitic hills everywhere crushed and broken into fearful precipices; whereas the sides turned to the north present plains of greater breadth, and dip at much smaller angles down towards the level country beyond. The great granitic boulders lie at their southern foot; and those specifically the same but of less magnitude, and transported the farthest off, are more worn and rounded. We have here, as elsewhere in the Penobscot bay, the evidence that it was the special business of the great denuding agent to cover the barren surface with soils, and that those soils are the result of local detritus—gravels, clays and sands crushed and ground from the detached rocks.

On the Taconic slates beyond these mountains, towards Ellsworth, we have the debris of the Taconic formation. Still beyond, through Dedham, we have a granitic formation, and the granitic boulders are in most wonderful profusion and of great magnitude. They were derived from the hills a little way toward the north. The same peculiarity may be said of North Haven above Vinalhaven. On that island, principally a trap region, you see trap boulders and rubbish. In the northern part of Vinalhaven where the Taconic slates are highly altered, you see boulders of the same character; on the granite below, granitic rocks; and still farther beyond, where the syenite has apparently been altered, the ruins of hornblendic rocks are found.

Around one of the quarries to the west of Carver's harbor, the ground is literally covered with boulders, some of which are enormous. After repeated attempts, I could not make out more than five per cent of foreign rocks among them. Many of these, turned out of their beds, exhibit the polishing and scratching of the common floor-rock of the island. Furthermore, if carefully turned over, we find some of them left just where they had last been employed in scratching the ledges, the parallel scratches of the boulder being placed parallel to those of the rock beneath. Of these foreign boulders we often have little or no evidence as to their origin. We have specimens of red and blue granite, trap, gneiss, mica schists, clay slates, and fossiliferous sandstones from the Katahdin region. We can well suppose them to have been dispersed by icebergs, or borne as freight to these localities by slowly moving glaciers. * * * * *

My conclusions, therefore, from the facts which I have enumerated, are, that a glacier once filled the basin between the Camden hills on the west, and those of Mount Desert on the east, forty miles wide—extended to a great distance north, involving several hills beside those mentioned of a thousand feet high, and certainly not less than three thousand feet thick.

Very truly yours,

December, 1862. JOHN DELASKI.

2. *Fossil Crustaceans from the Coal Measures and Devonian Rocks of British America;* by J. W. SALTER, (Q. J. Geol. Soc., xix, 75, and plate.) —The specimens described were furnished Mr. Salter by Dr. J. W. Dawson. The Devonian species are from St. Johns, New Brunswick; one is a small *Eurypterus;* for the other, of undetermined relations (but supposed to be possibly related to the Squilla group), the new genus *Amphipeltes* is instituted. The Carboniferous specimens are from the Joggins, Nova Scotia, and belong to two species, one a *Eurypterus*, the other regarded (and apparently with good reason) an *Amphipod*, and named *Diplostylus Dawsoni.*

3. *On the Cambrian and Huronian Formations;* by J. J. BIGSBY, (Q. J. Geol. Soc., xix, 36).—The author reviews in Part I. the facts with regard to the Cambrian, and, in Part II, those respecting the Huronian formation of Canada; and he concludes that the Huronian is not Cambrian, but more closely related to the Laurentian or Azoic.

4. *On the Lower Carboniferous Brachiopods of Nova Scotia;* by THOMAS DAVIDSON, (Q. J. Geol. Soc., xix, 158).—Besides remarks on the species under consideration, Mr. Davidson gives his opinion, as follows, with regard to the Paleozoic relations of the Permian. He says, that "Although there exist in the Permian formation some new forms which may recall to mind some which existed in the Mesozoic period, it must be allowed that that number forms the minority, and that, on the contrary, the great bulk of the Permian species, *to whatever class they may belong*, bears the most positive Paleozoic stamp, and that the species are in many cases the same that lived in the Carboniferous era, and some even in the Devonian."

5. *On fossil Estheriæ, and their distribution;* by T. RUPERT JONES, (Q. J. Geol. Soc., xix, 140).—The Ostracoid Crustaceans, called *Estheriæ* (formerly referred in part to the Molluscan genus *Posidonia*), are the subject, in this important paper, of brief general remarks, and several of the species of special criticism or elucidation. The species range from the Devonian, through the Carboniferous, Permian, and Triassic, to the Oolite and Wealden, and one species is queried as to its being Tertiary. They are supposed to be all either fresh-water or brackish-water. The *Estheriæ* of the Triassic beds of Pennsylvania, Virginia, and North Carolina are all referred by Mr. Jones to one species, for which he adopts Lea's name *E. ovata.* The *E. ovalis* of Emmons was published in the same year, but apparently in a later month of the year.

The fossil shells from the Carboniferous beds at Pottsville in Pennsylvania, named by Lea *Cypricardia Leidyi*, Mr. Jones regards as a new genus of Ostracoids, and names it *Leaia*, giving the species the name *Leaia Leidyi.* It is closely allied to a species from the Lower Carboniferous of Fifeshire, England.

6. *On a new Labyrinthodont Reptile, Anthracosaurus Russelli, from the Lanarkshire Coal-field;* by T. H. HUXLEY, (Q. J. Geol. Soc., xix, 56). —The fossil is a part of a skull, measuring 15 inches in length and nearly 12 in breadth. Besides this, there are vertebral bodies and a rib which probably belong to the *Anthracosaurus.* Professor Huxley regards the species as related to the Triassic *Mastodonsaurus.* He observes that the vertebræ closely resemble in section the vertebræ of the *Eosaurus* (from

the Joggins) described by Mr. Marsh; and he suggests that the *Eosaurian* vertebræ may have belonged to a Labyrinthodont, or to a species between a Labyrinthodont and an Ichthyosaurian. The best preserved rib is 6½ inches long and half an inch broad.

The two types of species which have been called Labyrinthodonts are that of the ARCHEGOSAURS [Ganocephala of Owen, but true *Ganoids* according to Agassiz], most abundant in the Carboniferous; and that of the MASTODONSAURS (genera, *Mastodonsaurus*, *Labyrinthodon*, *Capitosaurus*, *Trematosaurus*), common in the Triassic. The *Archegosaurs* had, as von Meyer has proved, a *persistent branchial apparatus*. Nothing is known as to whether this was true or not of the *Mastodonsaurs*. With respect to the *Denderpeton* and *Hylonomus* of Nova Scotia, discovered by Dawson, and recently referred to the *Archegosaur* group by Owen, and with regard also to the *Raniceps* of Wyman and the *Hylerpeton* of Owen, Professor Huxley remarks that it is not yet safe to decide whether their affinities are Archegosaurian or Mastodonsaurian.

7. *Anniversary Address before the Geological Society of London*, Feb. 20, 1863, by Prof. A. C. RAMSAY, President of the Society. 26 pp. 8vo. —At the annual meeting, before the Address of the President, the Wollaston Medal was awarded to Gustav Bischof of Bonn, and the Wollaston Donation-fund to Prof. Sneft. Prof. Ramsay, after brief notices of members deceased during the year—Richard Trench, Dr. C. C. v. Leonhard, Robert Raid, Rev. James Cumming, J. C. Nesbit, Dr. H. G. Bronn, B. de Doue, Dr. T. S. Trail and Marquis of Breadalbane,—takes up the topic of his discourse—Breaks in the succession of the British Paleozoic strata.

8. *On the production of crystalline limestone by heat.*—In this *Journal*, vol. xxxii, p. 112, an abstract is given of Rose's experiments on the deportment of carbonate of lime at a high temperature. Among other interesting conclusions drawn by Rose, he says that "chalk or compact limestone cannot be converted into crystalline limestone (or calc-spar) by exposure to a high temperature in closed vessels, and, as a general fact, that rhombohedral carbonate of lime is not formed in the dry way." Further, "that the so-called crystalline marble, obtained by Sir James Hall in his experiments, was probably nothing more than a slightly coherent but otherwise unaltered mass, which Hall erroneously considered to be crystalline marble."

Rose states, in a recent communication to the Berlin Academy of Sciences, that he was not entirely satisfied with his former results, especially as Dr. Horner, President of the Geological Society of London, assured him that he had inspected the specimen of marble made by Sir James Hall, and that it differed entirely from the amorphous product obtained in the Berlin experiments. Rose, therefore, repeated his investigations on the subject, and has now obtained results which differ entirely from those he formerly published, and which fully confirm the correctness of Sir James Hall's conclusion, that marble can be produced by exposing massive carbonate of lime to a high temperature under great pressure. The experiments were made with aragonite from Bilin, in Bohemia, and with lithographic limestone. In one case, the mineral was heated in a wrought-iron cylinder, and in the other, in a porcelain bottle, special precautions being taken to exclude the air, and make the vessels as near air-

tight as possible. These were exposed to a white heat for half an hour, and, on cooling, both the aragonite and the lithographic limestone were found to be converted into crystalline limestone, the former very much resembling Carrara marble, and the latter a grayish-white granular limestone. The change took place without any material decomposition, the resulting marble containing a trifle less carbonic acid than the lithographic limestone from which it was produced.—*Pogg. Ann.*, cxviii, 565. G. J. B.

9. *On the Flora of the Devonian Period in Northeastern America: Appendix;* by J. W. Dawson, LL.D., F.G.S., Principal of McGill University, Montreal. (Q. J. Geol. Soc., 1863. Read Dec. 17, 1862).—In a recent visit to Perry, the author (with the aid of Mr. Brown, of that place) thoroughly examined the present exposure of the plant-bearing bed. Among the specimens obtained were the following. (1.) Wood of a Conifer of the genus *Dadoxylon*. (2.) A new *Stigmaria* of the type of *S. exigua*. (3.) Specimens of *Lepidostrobus Richardsoni*, showing it to have been the fructification of a new and interesting species of *Lycopodites*. (4.) Another species of *Lycopodites* allied to *L. Erdmanni* Germar. (5.) A new species probably of the genus *Anarthrocanna* Gœppert. (6.) A new *Cordaites*. (7.) More perfect specimens of *Cyclopteris Browniana*, showing it to have been a large and beautiful flabellate leaf or frond, possibly identical with that from the Upper Devonian of Pennsylvania, figured by Prof. Rogers, in his *Pennsylvania Report*, vol. ii, part 2, pl. 22.[1] (8.) A Fern allied to *Cyclopteris Jacksoni*, but with a stem similar to that of *C. Roemeriana* Gœppert. (9.) New species of *Sphenopteris*, *Trichomanites* and *Carpolites*. (10.) Specimens of *Leptophlœum rhombicum*, showing its leaves and fructification. These, with some interesting specimens recently collected by Mr. R. Bell, of the Geological Survey, at Gaspé, Dr. Dawson hopes to describe in a future paper.

IV. BOTANY AND ZOOLOGY.

1. *Dimorphism in the Flowers of Linum.*—Referring back to our brief note upon the subject of dimorphous flowers in this *Journal* for Nov., 1862, and more particularly to Mr. Darwin's remarkable paper on the two sexual forms in *Primula* (in *Jour. of Linnæan Society*, no. 22), we wish now to call attention to some still more curious observations and experiments of Mr. Darwin, which were read to the Linnæan Society in February last, and are just published in the 26th no. of its *Journal*. The paper is entitled: "On the Existence of two forms, and on their reciprocal Sexual Relations, in several species of the genus *Linum*." The principal case is that of the crimson *Linum grandiflorum*, which is now common in gardens, and, as it flowers the whole summer long, is freely offered to the inspection of the curious. Dimorphic genitalia had hardly been noticed in the genus *Linum*. In the common cultivated Flax, it seems not to occur; but Planchon, in his Monograph, published 15 years ago, had noticed two or even three different states as to the relative length of the stamens and styles in *L. perenne* and *L. salsoloides*, and had even conjectured that this dimorphism might have some influence on the man-

[1] A portion of this figure is reproduced in *Dana's Manual of Geology*, in fig. 984, p. 750.

ner of fertilization. But this had been wholly overlooked "in such common garden-flowers as *L. grandiflorum* and *L. flavum*," until Mr. Darwin detected it, and worked out the case to the striking results which we record below, chiefly in his own words.

"The crimson *Linum grandiflorum* presents two forms, occuring in about equal numbers, which differ little in structure, but greatly in function. The foliage, corolla, stamens, and pollen (examined dry and distended with water) are alike in both forms. The difference is confined to the pistil: in the one form, which I will call "short-styled," the column formed by the united styles and the short stigmas together is about half the length of the whole pistil in the other and "long-styled" form. A more important distinction is, that the five stigmas in the short-styled form diverge greatly from each other, and pass out between the filaments of the stamens, and thus lie within the tube of the corolla. In the long-styled form the elongated stigmas stand nearly upright and alternate with the anthers. In this latter form, the length of the stigmas varies considerably, their upper extremities projecting even a little above the anthers, or reaching up only to about their middle. Nevertheless, there is never the slightest difficulty in distinguishing between the two forms; for, besides the difference in divergence, the stigmas of the short-styled form never reach even to the bases of the anthers. In the short-styled, the papillæ on the stigmatic surfaces are shorter, darker-colored, and more crowded together than in the long-styled form: but these differences seem due merely to the shortening of the stigma; for, in the varieties of the long-styled form with shorter stigmas, the papillæ are more crowded and darker-colored than in those with the longer stigmas. Considering the slight and variable differences between the two forms of this *Linum*, it is not surprising that they have been hitherto overlooked.

"In 1861, I had eleven plants growing in my garden, eight of which were long-styled, and only three short-styled. Two very fine long-styled plants grew in a bed a hundred yards off, and separated from the others by a screen of evergreens. I marked twelve flowers, and put on their stigmas a little pollen from the short-styled plants. The pollen of the two forms is, as stated, identical in appearance; the stigmas of the long-styled flowers were already thickly covered with their own pollen,—so thickly that I could not find one bare stigma; and it was late in the season, namely, September 16th. Altogether, to expect any result from this trial seemed almost childish. From my experiments, however, on *Primula*, which have been laid before this Society (*Journal*, vi. 77), I had faith, and did not hesitate to make the trial, but certainly I did not anticipate the full result. The germens of these twelve flowers all swelled, and ultimately six fine capsules (the seed of which germinated this year) and two poor capsules were produced; only four capsules shanked off. These two plants produced, before and after and at the time of the trial, a vast number of flowers, but the germens of not even one swelled. All these flowers, though their stigmas were so densely covered with their own pollen, were absolutely barren.

"The nine other plants, six long-styled and three short-styled, grew in the beds of the same flower-garden. Four of the long-styled produced no seed-capsules; one produced two; but the remaining long-styled plant grew so close to a short-styled plant that their branches touched; and this produced twelve capsules, but they were poor. The case was different with the short-styled plants. The plant which grew in juxtaposition with the long-styled plant produced ninety-four imperfectly fertilized capsules, containing a multitude of bad seeds, with a moderate number of good seeds The two other short-styled plants grew in a single clump, and were very small, being partly smothered by other plants; they did not stand very close to any long-styled plants, yet they yielded together nineteen capsules. These facts seem to show that the short-styled plants are far more fertile with their own pollen than

the long-styled. We shall immediately see that this is the case in a slight degree. But I suspect that in this instance the difference in fertility between the two forms was in part due to a distinct cause. I repeatedly watched the flowers, and only once saw a humble-bee momentarily alight on one, and then fly away, as if it were not to its taste. If bees had visited the several plants, there cannot be a doubt that the four long-styled plants which did not produce a single capsule would have borne an abundance. But several times I saw small Diptera sucking the flowers; and these insects, though not visiting the flowers with anything like the regularity of bees, would carry a little pollen from one form to the other, especially when growing close together; and the stigmas of the short-styled plants, diverging within the tube of the corolla, would be more likely than the upright stigmas of the long-styled to receive a small quantity of pollen when brought by small insects. From the much greater number of long-styled than of short-styled flowers in the garden, evidently the short-styled would be more likely to receive some pollen from the long-styled, than the long-styled from the short-styled.

"In 1862, I raised thirty-four plants of this *Linum* in a hotbed; and these consisted of seventeen long-styled and seventeen short-styled forms. Seed sown later in the flower-garden yielded seventeen long-styled and twelve short-styled forms. These facts justify the statement that the two forms are produced in about equal numbers. The first thirty-four plants were kept under a net which excluded insects. I fertilized heteromorphically fourteen long-styled flowers with pollen from the short-styled, and got eleven fine seed-capsules; these contained on an average 8·6 seeds per capsule, but only 5·6 were apparently good. It may be well to state that ten seeds is the maximum possible production for a capsule, and that our climate cannot be very favorable to this North-African plant. On three occasions, I fertilized homomorphically the stigmas of altogether nearly a hundred flowers (but did not separately mark them) with their own pollen, but taken from separate plants, so as to prevent any possible ill effects from close interbreeding; and many other flowers were produced, which, as before stated, would get plenty of their own individual pollen; yet from all these flowers, borne by the seventeen long-styled plants, only three capsules were produced; one of these included no seed, and the other two together gave only five good seeds. Nor do I feel at all sure that this miserable product of the two half-fertile capsules from the seventeen plants, each of which must have produced at least fifty or sixty flowers, is really the result of their fertilization by their own pollen; for I made a great mistake in keeping the two forms under the same net, with their branches often interlocking; and it is surprising that a greater number of flowers were not accidentaly fertilized.

"Of the short-styled flowers, I fertilized heteromorphically twelve with the pollen of the long-styled (and to make sure of the result I previously castrated the majority), and obtained seven fine seed-capsules. These included an average of 7·6 seeds, but of apparently good seed only 4·3 per capsule. At three separate times, I fertilized homomorphically nearly a hundred flowers with their own-form pollen, taken from separate plants; and numerous other flowers were produced, many of which must have received their own pollen. From all these flowers borne on the seventeen plants, only fifteen capsules were produced, of which only eleven contained any good seed, on an average 4·2 per capsule. As remarked in the case of the long-styled plants, some even of these capsules were perhaps the product of a little pollen accidentally fallen from the flowers of the other form. Nevertheless, the short-styled plants seem to be slightly more fertile with their own pollen, in the proportion of fifteen capsules to three, than the long-styled: the real proportional excess in fertility is probably a little greater, as the short-styled flowers, when not disturbed, do not so surely receive their own pollen as do the long-styled. The greater self-fertility of the short-styled flowers was, as we have seen, also

shown by the plants left to themselves, and but sparingly visited by insects, in the flower-garden in 1861, and likewise by those raised in 1862."

Next, with the view of ascertaining the immediate cause of this almost absolute sterility of long-styled pistils with their own form of pollen, and in a less degree of short-styled pistils with their own form of pollen, a series of experiments was made, in which pollen of either sort was applied to the stigmas of either sort, and the stigmas were dissected under the microscope, after an interval of 24 hours or less. When pollen of a short-styled flower was applied to the stigmas of a long-styled, and likewise when, conversely, that of a long-styled flower was applied to the stigmas of a short-styled, a microscopical dissection showed that the stigmas were freely penetrated by numerous pollen-tubes. But when homomorphic unions were attempted, no pollen-tubes, or scarcely any, were emitted; even after an interval of three days the stigmas remained straight and fresh-colored, and the pollen inactive. When two or three of the stigmas were dusted with their own form of pollen, and the others with the opposite form, the difference was striking; the former stigmas remaining straight, fresh, and unpenetrated or nearly so, while the latter were soon discolored, twisted, half-shrivelled, and penetrated by a multitude of pollen-tubes.

"This seems to me a remarkable physiological fact. The pollen-grains of the two forms are undistinguishable under the microscope; the stigmas differ only in length, degree of divergence, and in the size, shade of color, and approximation of their papillæ, these latter differences being variable and apparently simply due to the elongation of the stigma. Yet we plainly see that the two pollens and the two stigmas are widely dissimilar in action, the stigmas of each form being almost powerless on their own pollen, but causing, through some mysterious influence, by simple contact (for I could detect no viscid secretion), the pollen-grains of the opposite form to protrude their tubes. It may be said that the two pollens and the two stigmas by some means mutually recognize each other. Taking fertility as the criterion of distinctness, it is no exaggeration to say that the pollen of the long-styled *Linum grandiflorum* (and conversely of the other form) has been differentiated, with respect to the stigmas of all the flowers of the same form, to a degree corresponding with that of distinct species of the same genus, or even of species of distinct genera."

The results are nearly the same in *L. perenne*, except that pollen-tubes were found to be produced in attempted homomorphic unions, but either they did not reach the ovules, or they did not act on them. "Neither pollen when placed on its own stigma causes fertility, except occasionally and in a very moderate degree." The following remarks neatly discriminate between the action of the wind and that of insects in carrying pollen. And then the twisting of the long styles in *L. perenne* and the divergence of the short ones in both species are noteworthy:

"Botanists, in speaking of the fertilization of plants or of the production of hybrids, often refer to the wind or to insects as if the alternative were indifferent. This view, according to my experience, is entirely erroneous. When the wind is the agent in carrying pollen, either from one separated sex to the other, or from hermaphrodite to hermaphrodite (which latter case seems to be almost equally important for the ultimate welfare of the species, though occurring perhaps only at long intervals of time), we can recognize structure as manifestly adapted to the action of the wind as to that of insects when they are the carriers. We see adaptation to the wind in the incoherence of the pol-

len, in the inordinate quantity produced (as in the Coniferæ, Spinage, &c.), in the dangling anthers, well fitted to shake out the pollen, in the absence or small size of the perianth, or in the protrusion of the stigmas at the period of fertilization, in the flowers being produced before they are hidden by the leaves, in the stigmas being downy or plumose (as in the Gramineæ, Docks, and other plants) so as to secure the chance-blown grains. In plants which are fertilized by the wind, the flowers do not secrete nectar, their pollen is too incoherent to be easily collected by insects, they have not bright-colored corollas to serve as guides, and they are not, as far as I have seen, visited by insects. When insects are the agents of fertilization (and this is incomparably the more frequent case, both with plants having separated sexes and with hermaphrodites), the wind plays no part, but we see an endless number of adaptations to ensure the safe transport of the pollen by the living workers. We can recognize these adaptations most easily in irregular flowers; but they do not the less occur in perfectly regular flowers, of which those of *Linum* offer an instance, as I will almost immediately endeavor to show.

"I have already alluded to the rotation of each separate stigma in the long-styled form alone of *Linum perenne*. In the other species examined by me, and in both forms when the species are dimorphic, the stigmatic surfaces face the centre of the flower, and the furrowed backs of the stigmas, to which the styles are attached, face the circumference. This is the case, in the bud, with the stigmas of the long-styled flowers of *L. perenne*. But, by the time the flower in this form has expanded, the five stigmas, by the torsion of that part of the style which lies beneath the stigma, twist round and face the circumference. I should state that the five stigmas do not always perfectly turn round, two or three often facing only obliquely towards the circumference. My observations were made during October; and it is not improbable that earlier in the season the torsion would have been more perfect; for after two or three cold and wet days the movement was very incomplete. The flowers should be examined shortly after their expansion; for their duration is brief, and, as soon as they begin to wither, the styles become spirally twisted together, and the original position of the parts is lost.

"He who will compare the structure of the whole flower in both forms of *L. perenne* and *grandiflorum*, and, I may add, of *L. flavum*, will, I think, entertain no doubt about the meaning of this torsion of the styles in the one form alone of *L. perenne*, as well as the meaning of the divergence of the stigmas in the short-styled forms of all three species. It is absolutely necessary, as we now know, that insects should reciprocally carry pollen from the flowers of the one form to those of the other. Insects are attracted by five drops of nectar, secreted exteriorly at the base of the stamens, so that to reach these drops they must insert their proboscides outside the ring of broad filaments, between them and the petals. In the short-styled form of the above three species, the stigmas face the axis of the flower; and had the styles retained their original upright and central position, not only would the stigmas have presented their backs to insects as they sucked the flowers, but they would have been separated from them by the ring of broad filaments, and could never have been fertilized. As it is, the styles diverge greatly and pass out between the filaments. The stigmas, being short, lie within the tube of the corolla; and their papillous faces, after the divergence of the styles, being turned upwards, are necessarily brushed by every entering insect, and thus receive the required pollen.

"In the long-styled form of *L. grandiflorum*, the parallel anthers and stigmas, slightly diverging from the axis of the flower, project only a little above the tube of the somewhat concave corolla; and they stand directly over the open space leading to the drops of nectar. Consequently, when insects visit the flowers of either form (for the stamens in this species occupy the same position in both forms), they will get their proboscides well dusted with the

coherent pollen. As soon as the insect inserts its proboscis to a little depth into the flower of the long-styled form, it will necessarily leave pollen on the faces and margins of the long stigmas; and as soon as the insect inserts its proboscis to a rather greater depth into the short-styled flowers, it will leave pollen on their upturned stigmatic surfaces. Thus the stigmas of both forms will indifferently receive the pollen of both forms; but we know that the pollen of the opposite form alone will produce any effect and cause fertilization.

"In the case of *L. perenne*, affairs are arranged a little more perfectly; for the stamens in the two forms stand at different heights, and pollen will adhere to different parts of an insect's body, and will generally be brushed off by the stigmas of corresponding height, to which stigmas each kind of pollen is adapted. In this species, the corolla is flatter, and in the one form the stigmas and in the other form the anthers stand at some height above the mouth of the corolla. These longer stigmas and longer stamens do not diverge greatly; hence insects, especially rather small ones, will not insert their proboscides between the stigmas or between the anthers, but will strike against them, at nearly right angles, with the back of their head or thorax. Now, in the long-styled flowers of *L. perenne*, if each stigma had not rotated on its axis, insects in visiting them would have struck their heads against the backs of the stigmas; as it is, they strike against the papillous fronts of the stigmas, and, their heads being already charged with the proper coherent pollen from the stamens of corresponding height borne by the flowers of the other form, fertilization is perfectly effected. Thus we can understand the meaning of the torsion of the styles in the long-styled flowers alone, as well as their divergence in the short-styled flowers."

Linum Lewisii is inferred to be distinct as a species from *L. perenne* on the ground of Planchon's remark, that the styles are in the same specimen sometimes equalling, sometimes shorter, and sometimes longer than the stamens. The inference may not be correct: and although the plant is said to extend northward even to the shores of the Arctic sea, it does not specially belong to the "Arctic Zone," but abounds on the Western plains.

A remark in this interesting paper lets us know that Mr. Darwin has detected *trimorphism*, viz: three distinct forms, each of which produces two kinds of pollen, in *Lythrum Salicaria*, neither pollen when placed on its own stigma producing fertility, "except occasionally and in a very moderate degree; yet the pollen-tubes in each case freely penetrate the stigmatic tissue." Here the number of heteromorphic and therefore fertile unions possible is largely increased. These degrees of sterility in homomorphic unions,—from complete inertness of the pollen to the occasional production of an inefficient pollen-tube, to the copious production of inefficient pollen-tubes penetrating the style, and to the occasional fertilization of an ovule,—are very noteworthy, and Mr. Darwin will some day turn them to account in his own way. "*Natura non agit saltatim.*" Let us add, in conclusion, that when such fine biological discoveries are so readily made by the study of some of the commonest plants, no botanical student, however restricted his range, need slumber for lack of occupation, nor suppose that the field is exhausted. Out of old fields, indeed, not only comes all this new corn from year to year, but such gleanings as these are richer far in interest than any crop of new species from a virgin soil. A. G.

2. *Variation and Mimetic Analogy in Lepidoptera.*—Mr. Bates (whose interesting book of travels, *The Naturalist on the River Amazon*, is exciting much attention in England, and which we trust will be reprinted here) has contributed an elaborate paper to the *Transactions of the Linnæan Society*, vol. xxiii (1862), entitled *Contributions to the Insect Fauna of the Amazon Valley, Lepidoptera, Heliconidæ.* The materials were gathered by the author during eleven years of travel and research in the Amazon region. The introduction to this paper treats, among other subjects, most largely of the highly curious one above referred to, i. e., the extraordinary mimetic resemblance which certain butterflies present to other butterflies belonging to distinct groups. There are also collections of pregnant facts upon variation and divergence into races. We had marked many pages for extract; but room has not been found for them. It seems less needful to copy large parts of Mr. Bates' narrative now, since a good abstract of his paper has recently appeared in the *Natural History Review.* The bearing of Mr. Bates' observations may be inferred from the remark of his reviewer: "it is hardly an exaggeration to say, that, whilst reading and reflecting on the various facts given in this memoir, we feel ourselves to be as near witnesses as we can ever hope to be of the creation of a new species on this earth." The two subjects, *variation* and *simulation*, as may be inferred, are considered in respect to their bearing upon Mr. Darwin's theory, of which Mr. Bates is a zealous upholder, although his observations were made before this celebrated theory was promulgated. The facts set forth about variation appear excellently to illustrate the formation of races and nearly related species through gradual divellent variation; and those on mimetic analogy are not only wonderfully curious, but are most ingeniously applied to illustrate the doctrine of natural selection, under a peculiar phase.

We will first notice some of the reported facts about *variation.* Such an amount and such gradations of variability as Mr. Bates reports of butterflies, we have ceased to think very extraordinary in the vegetable world; yet we had been led to suppose that forms in the animal world were everywhere more definite and fixed. But Mr. Bates' observations seem to have convinced him "that there is a perfect gradation in variability, from butterflies of which hardly two can be found alike, to slight varieties, to well marked races, to races that can hardly be distinguished from species, to true and good species." In the genus *Ceratinia*, for instance, those parts of structure [i. e. the veining of the wings] which form fixed generic characters in other groups are here variable in the sexes, and in individuals of the same sex. *C. Ninonia* "evidently varies in different ways in different localities; yet the local varieties are not definite, the segregation of the races is not complete: so that it is embarrassing to decide whether to treat the form as one polymorphic species, including the variations under one and the same definition, or to describe separately the type and the local varieties. Besides these incomplete local modifications, easily traceable to the type, there are, as often happens in the case of prolific, widely distributed, and variable species, a number of other forms rather more strongly marked and better defined, which inhabit regions rather more distant from the locality of the type than those which the mere varieties inhabit. These are admitted on all hands to be distinct species; but I think it would be difficult to prove

that these were not also varieties of *C. Ninonia*, which have become more completely segregated from the parent form." The examples are given. This is essentially what De Candolle concludes of Oaks, as we have seen in a former article. *Mechanitis Polymnia* affords one of the most striking cases. The typical form of the perfect insect, as figured by Cramer, prevails at Para and elsewhere in the region of the lower Amazon. There all the specimens are very much alike: while at Ega, on the Upper Amazon, very few individuals conform to the Cramerian type. Among the numerous forms, one, which he names *M. Egaënsis*, predominates; but all the intermediate forms between it and the typical *M. Polymnia* occur there, only in fewer numbers. At St. Paulo, 260 miles further west, the species was again extremely variable, some individuals coming near the type, but none identical with it. The varieties were quite different from those of Ega; the *M. Egaënsis* was wholly absent, but a new variety abounded, of which there was no trace at Ega; this has been figured and described as a distinct species. The complete set of connecting forms convinced the observer that all belonged to one species, disseminated over a large area, and modified in certain districts. He affirms that the varieties were of such a nature, as to form and colors, that they could not be thought to be hybrids between two or more distinct species. And also, that the amount of local modification in no way accorded with obvious differences in the local conditions; for the species was, on the one hand, totally changed within 260 miles of very similar soil and climate, but, on the other hand, was constant in districts 600 miles apart and very different in physical conditions. Extending the view up to the eastern slopes of the Andes, there are said to be still other forms, some of them clearly varieties of *M. Polymnia*, although they have been described as species; others more sharply defined and having the appearance of true species. So Mr. Bates thinks that,—

"The conclusion is unavoidable that these apparently distinct species are modifications, as well as the undoubted varieties are; for we have the species in all stages of modification,—simple variation, local variety scarcely distinguishable from a mere variation, complete local variety, and well marked race or species. The forms of *M. Polymnia* found in South Brazil confirm this view. At Rio Janeiro the well marked race or species *M. Lysimnia* alone is found; at Bahia (traveling towards the home of the type, *M. Polymnia*), *M. Lysimnia* in company with *M. Nesæa;* at Pernambuco (further northward) *M. Nesæa* alone occurs; at Pará this form is seen no more, and *M. Polymnia* in its typical dress monopolizes the field. These facts seem to teach us that, in this and similar cases, a new species originates in a local variety, where the conditions are more favorable to it than to the typical form, and that a large number of such are simultaneously in progress of formation from one variable and widely distributed species. The new species cannot be proved to be established as such, unless it be found in company with a sister form which has had a similar origin, and maintaining itself perfectly distinct from it. Cases of two extreme varieties of a species being thus brought into contact by redistribution or migration, and not amalgamating, will be found to be numerous when the subject is inquired into." . . . . "It is an advantage to a form to have a sphere of life different from its allies; when two sister forms keep themselves distinct in a locality, it is a sign that they have acquired sufficient difference to fill two separate spheres. If they paired together they would soon become one again. Nature may be said to place a premium on diversity; for she thus destroys the incompletely formed race, and preserves the completely formed one. The case of *Mechanitis Polymnia* differs from that of

Leptalis Theonöe in exhibiting the production, generally, of only one local form in a district, instead of many. As far as my observations go, this seems to have been the most frequent course in nature. More than one new race would with difficulty be formed in a limited area, when the individuals live in close neighborhood, except in such cases as our *Leptalis*, where rigid destruction of intermediate forms is going on, thus restricting the choice of mates to the surviving forms; or in such genera as *Ithomia*, where there is no doubt the insects carefully select their exact counterparts in pairing."

In the latter case, where each sort strictly interbreeds, the races once originated would be kept distinct as long as they existed. Mr. Bates asserts that in the *Ithomiæ* and allied genera, "where a number of very closely allied species fly together, they keep themselves perfectly distinct, there are no hybrid forms, and, on observing individuals *in copula*, I almost always found the pair to be precisely the same in color and markings." The exception was in *Mechanitis Polymnia*, above mentioned, "a polymorphic species, whose local varieties are in an imperfect state of segregation." This pairing of exact counterparts would—upon principles which we have particularly explained in this Journal—accelerate the diversification or divergence into races, by enabling each advance of variation to be held. And it would, as Mr. Bates remarks, enable a number of closely allied forms to exist, either together or in contiguous areas, without amalgamating.

In his remarks upon *Mechanitis Polymnia*, as illustrating the course apparently followed by nature in the formation of local species, the author repeats that:—

"We find, in this most instructive case, all the stages of the process, from the commencement of the formation of a local variety (var. *Egaënsis*) to the perfect segregation of one (var. *Lysimnia*) considered by all authors as a true species. In this species, most of the local varieties are connected with their parent form by individuals exhibiting all the shades of variation; and it is on this account only that we know them to be varieties. In the species allied to *Ithomia Flora*, the forms are in a complete state of segregation (with the exception of *I. Illinissa*, which throws light on the rest), and therefore they are considered as species; they are, in fact, perfectly good species, like all other forms considered as such in natural history. It is only by the study of variable species that we can obtain a clue to the explanation of the rest. But such species must be studied in nature, and with strict reference to the geographical relations of their varieties. Many closet naturalists, who receive disconnectedly the different varieties of any group, treat them all as independent species; by such a proceeding, it is no wonder that they have faith in the absolute distinctness and immutability of species."

The *mimetic analogies*, of which many of the *Heliconidæ* are the objects, have been mentioned by modern authors who have written on the group; "but no attempt has been made to describe them fully or to explain them." Mr. Bates exhibits the more striking cases in a tabular view, which gives some idea of the extent to which this imitation prevails, and, of the various tribes of Lepidoptera to which the imitators belong. It is concluded that the *Heliconidæ* are the *imitated*, because they have all the same family aspect, while the imitators or analogous species are dissimilar to their nearest allies,—are perverted, as it were, from the facies of the group to which they severally belong.

"The resemblance is so close that it is only after long practice that the true can be distinguished from the counterfeit when on the wing in their native

forests. I was never able to distinguish the *Leptalides* from the species they imitated, although they belong to a family totally different in structure and metamorphosis from the *Heliconidæ*, without examining them closely after capture. They fly in the same parts of the forest, and generally in company with the species they mimic. I have already given an account of the local modifications to which the *Heliconidæ* are subject. It is a most curious circumstance that corresponding races or species of counterfeiting groups accompany these local forms. In some cases I found proof that such species are modified from place to place to suit the peculiar forms of *Heliconidæ* there stationed."

The details in evidence of this are fully explained and illustrated by plates. Nothing can be more curious. The *Ithomiæ* imitated are excessively numerous in individuals; the imitating *Leptalides* are rare, not more than one to a thousand of the other. The latter has not been found in any other district or country than in those inhabited by the *Ithomiæ* which they counterfeit. The resemblance is often carried to minutiæ, such as the color of the antennæ and the spotting of the abdomen. Not only are the *Heliconidæ* thus imitated; some of them are themselves imitators, i. e., they counterfeit each other, species belonging to distinct genera having been confounded, owing to their close resemblance in coloring and marking.

"These imitative resemblances, of which hundreds of instances could be cited, are full of interest, and fill us with the greater astonishment the closer we investigate them; for some show a minute and palpably intentional likeness which is perfectly staggering. I have found that those features of the portrait are most attended to in nature which produce the most effective deception when the insects are seen in nature."

Similar imitations are said to occur in the Old World, in other families of Butterflies and Moths; but no instance is known of a tropical species of one hemisphere counterfeiting a form belonging to the other. Other orders of insects supply such cases in certain families. "Many instances are known where parasitic bees and two-winged flies mimic in dress various industrious or nest-building bees, at whose expense they live in the manner of the Cuckoo. I found on the banks of the Amazon many of these Cuckoo bees and flies, which all wore the livery of working bees peculiar to the country." Mr. Wallace has noticed two similar and equally striking instances of mimicry in birds.

Now, as to the final cause of these mimetic analogies,—

"When we see a species of Moth which frequents flowers in day-time wearing the appearance of a Wasp, we feel compelled to infer that the imitation is intended to protect the otherwise defenseless insect by deceiving insectivorous animals which persecute the moth and avoid the wasp. May not the Heliconide dress serve the same purpose to the *Leptalis*? Is it not probable, seeing the excessive abundance of the one species, and the fewness of the other, that the Heliconide is free from the persecution to which the Leptalis is subjected?" . . . .

"I believe that the specific mimetic analogies exhibited in connection with the *Heliconidæ* are adaptations,—phenomena of precisely the same nature as those in which insects and other beings are assimilated in superficial appearance to the vegetable or inorganic substances on which, or amongst which, they live. The likeness of a Beetle or a Lizard to the bark of the tree on which it crawls cannot be explained as an identical result produced by a common cause acting on the tree and the animal."

A full series of such imitations by insects, both of inanimate and of living objects, is then given. That such imitative resemblances as we are considering are of the same class as these, and subject to the same explanation, is obvious from the fact of one species mimicking an inanimate object, while one of an allied genus imitates an insect of another family. They are all evidently "adaptations having in view the welfare of the creatures that possess them." Every species maintains its hold upon existence only through some endowment enabling it to withstand the various adverse circumstances to which it is exposed; and the means are of endless diversity,—organs of offense, great fecundity, capabilities for wide dispersion, and, among the rest, the adaptive resemblances of a defenseless species to one which enjoys some kind of protection. The multitudinous swarms of slow-flying *Heliconidæ* on the Amazon, apparently defenseless, must enjoy some immunity from the insectivorous animals. Mr. Bates never saw them preyed upon by birds or Dragon-flies, or molested by Lizards when at rest; and their dead bodies set out to dry were rarely attacked by vermin. They all have a peculiar smell. So it is probable that they are unpalatable to insect enemies. "If they owe their flourishing existence to this cause, it would be intelligible why the *Leptalidæ*, whose scanty number of individuals reveals a less protected condition, should be disguised in their dress and thus share their immunity."

This naturally leads to Mr. Bates' explanation of the process by which these mimetic resemblances and other such adaptations are brought about. The admirer of *natural selection* finds here a beautiful application of the principle. Given the *Heliconidæ* as they are, segregated and in course of segregation into variations, varieties, races, and species under conditions of natural selection which are still occult, and supposing (what their flourishing numbers prove) that their taste, odor, or something else, affords a comparative immunity from the attacks of their natural enemies, the existence of their more exposed analogues, in each locality, would seem to depend upon the closeness of their resemblance to the protected *Heliconidæ* of the district, such resemblance being apparently the only means of escaping extermination by insectivorous animals. As the imitated species vary from place to place, so must the imitators if they would retain their hold upon life. And, of all the variations which are constantly arising, only those which do resemble the protected form near enough to deceive the insectivorous enemy, will retain their hold. This is natural selection, the insectivorous animals being the selecting agents; and the operation proceeds to draw out steadily, in certain favorable directions, the suitable variations which arise from generation to generation, as a result of the extermination of those sorts or varieties which are not enough like the protected species to deceive the enemy.

"If a mimetic species varies, some of its varieties must be more and some less faithful imitations of the object mimicked. According therefore to the closeness of its persecution by enemies, who seek the imitator but avoid the imitated, will be its tendency to become an exact counterfeit,—the less perfect degrees of resemblance being, generation after generation, eliminated, and only the others left to propagate their kind." "The fact of one of the forms

of *Leptalis Theonoë*, namely *L. Lysinoë*, mimicking an Ega, not an *Ithomia*, but a flo urishing species of another quite distinct family (*Stalachtis Duvalii*), shows that the object of the mimetic tendencies of the species is simply disguise, and that, the simple individual differences in that locality being originally in the direction, not of an *Ithomia*, but of another object equally well answering the purpose, selection operated in the direction of that other object." "When the persecution of a variable local form of our *Leptalis* is close or long continued, the indeterminate variations naturally become extinct; nothing then remains in that locality but the one exact counterfeit, whose exactness, it must be added, is henceforth kept up to the mark by the insect pairing necessarily with its exact counterpart, or breeding in and in. This is the condition of *L. Theonoë*, &c. . . . . When (as happens at St. Paulo, where a greater number of individuals and species, both of *Ithomia* and *Leptalis*, exists . . . .) many species have been in the course of formation out of the varieties of one only, occasional intercrossing may have taken place; this would retard the process of segregation of the species, and, in fact, aid in producing the state of things (varieties and half-formed species) which I have already described as there existing." "Such, I conceive, is the only way in which the origin of mimetic species can be explained. I believe the case offers a most beautiful proof of the truth of the theory of natural selection. It also shows that a new adaptation, or the formation of a new species, is not effected by great and sudden change, but by numerous small steps of variation and selection."

At a time like the present, when the notion that species are derivative, somehow or other, is received as the most probable opinion by such an increasing number of competent observers and thinkers—including, it may be added, the names of Lyell and of Owen,—and when it appears to the thoroughly conservative and well-informed President of the Linnæan Society[1] "that the tide of opinion among philosophical naturalists is setting fast in favor of Mr. Darwin's hypothesis," such illustrations of the latter as Mr. Bates has presented are worthy of attentive consideration. But we need not agree with Mr. Bates in his conclusion that the impression produced "of there being some innate principle in species which causes an advance in organization in a special direction," so that the result is "a predestined goal," is untenable, and the appearances which suggest such idea, illusory. Because variations are picked out, preserved, and led to useful ends by natural selection, it does not follow, nor has it ever been shown, that they occur lawlessly and at random.

A. G.

3. *Flora Australiensis: a Description of the Plants of the Australian Territory;* by GEORGE BENTHAM, F.R.S., P.L.S., assisted by FERDINAND MÜLLER, M.D., F.R.S. & L.S., Government Botanist, Melbourne, Victoria, vol. I. (*Ranunculaceæ* to *Anacardiaceæ*.) London: Reeve & Co.

[1] Address of George Bentham, Esq., President, read at the Anniversary Meeting of the Linnæan Society, May 25, 1863. Published at the request of the Fellows. It is mainly a critical review of the recent progress of biological (i. e. in its properest sense, physiological) science, and is in almost every respect well-considered and forcible. In referring to Professor Wyman's paper, in this Journal, on the production of Infusoria, Mr. Bentham, probably relying upon others, has failed to appreciate the thorough care, appositeness, and simplicity of his experiments,—which, as we judge, stand at an advantage over Pasteur's,—especially in the very point remarked on, viz: the degree of heat applied. This was not only considerably higher in some of Wyman's experiments than in Pasteur's, but must have been far more efficient, as it was not exposure to dry heat, but boiling.

1863, pp. 508, 8vo.—This is the first volume of another of those Floras of British Colonies, published under the authority of the Home Government, and in the present instance, we believe, mainly at the expense of the colonies concerned, upon a plan arranged by Sir Wm. Hooker, in connexion with Mr. Bentham and Dr. Hooker. The Flora of Hongkong, by Mr. Bentham, was the first of the series. This related to a very small district. The present embraces vast regions and a preëminently interesting and peculiar vegetation. So immense is this undertaking that, with all his vast resources and preparation, and all his courage and aptitude, we can hardly suppose that Mr. Bentham would have taken it in hand, were it not for the coöperation of his colleague in this work, Dr. Ferdinand Müller, who has already done so much for the Australian Flora. While we with pleasure see Dr. Müller's name upon the title-page of this work, it is satisfactory to find that his important aid is offered and secured without the drawbacks which are inseparable from joint authorship, except where the parties can literally work side by side. Mr. Bentham is here undividedly responsible for the execution of the present volume. He has assumed the task with alacrity, prosecuted it thus far with his wonted vigor, and, from his wonderful knack of carrying through to completion whatever he undertakes, we may confidently hope that the botanists of each and every Australian colony may within a few years have furnished to his hands a Flora as perfect for its purpose as is the Handbook of the British Flora itself.

The excellent Outlines of Botany with special reference to Local Floras, which was prepared originally for the Handbook of the British Flora, is prefixed to the present as well as to the Hongkong Flora, having undergone studied revision. We notice that the radicle, which in the first instance was spoken of as "the future root," is now called "the base of the future root,"—expressing no opinion as to its morphological nature. In the next Colonial Flora, however, we confidently expect that it will be called the primordial internode, upon the node, i. e. the summit, of which the cotyledons are inserted,—a view which obviously suggests itself to the morphologist, and which, as we suppose, may be demonstrated by its position, its growth, and its structure. We know of nothing which is true of the internode next above the cotyledons which is not also true of that below them. In the first edition of these Outlines, "all the pistils of a flower" are spoken of, and the word is used as Linnæus used it. But in the revision, the author falls back to the Tournefortian use. This is a question of terminology, upon which opinions may fairly be divided: but we side with Linnæus. A. G.

4. *Notes on the Loranthaceæ, with a Synopsis of the Genera;* by Daniel Oliver, F.L.S., Professor of Botany, University College, London. —An important paper, read to the Linnæan Society in January last and recently printed in its *Journal*, No. 26. Prof. Oliver repudiates Mr. Miers' attempt to establish *Viscum* and its near allies as a separate order, wishes to unite *Santalaceæ* with the *Loranthaceæ*, and would even follow Baillon in adding the *Olacineæ*,—all of these clearly belonging to one natural group, the great divisions of which are kept apart mainly, it would seem, because of their wide separation in the Candollean sequence of orders. Some genuine species of *Phoradendron* are found to have the

anthers one-celled by confluence: indeed Prof. Oliver has found them so in some specimens of our North American Mistletoe, *P. flavescens*. No. 1125 of Fendler's Venezuela collection is found to belong to Pœppig's genus, *Antidaphne*. The bracts or scales subtending flowers in the genus *Lepidoceras* of Dr. Hooker are found to persist as the apex of a true lamina of a leaf, which is subsequently developed by a growth of its base in a very curious mannner. The scale does duty first as a bract, and afterwards, by a basal growth, the insertion or petiolar portion of this scale becomes a green leaf. The true Loranthaceous genera here admitted, and succinctly defined, are twelve, with indications that *Eremolepis Wrightii* of Grisebach, one of Mr. C. Wright's discoveries in Cuba—may be the type of a thirteenth genus. A. G.

5. *Parthenogenesis in Plants.—A presumed Case of Parthenogenesis in a species of Aberia*, a Bixaceous genus, reported by Dr. T. Anderson to the Linnæan Society, is recorded in the *Journal* of the proceedings of that Society, No. 26. One or two bushes of the species (of unknown origin) flowered in the Calcutta Botanical Garden in the year 1861. "They were female plants, no stamens were detected, yet they bore a large crop of well ripened fruits." "The seeds obtained from these plants were sown, and there is now a vigorous stock of young plants." In 1862 the same plant flowered again, and during a month produced only pistilliferous flowers. "From the opening of the first flower-bud until the last withered flower dropped off, not a day passed without a careful examination being made by me for the traces of a stamen in the flowers, but without finding one." The fruit set from many of the ovaries; but the tree was soon after destroyed in a gale. The evidence is not wholly complete; but as far as it goes it confirms the case of *Cœlebogyne*.

A. G.

6. *Structure and Fertilization of certain Orchids.*—In this *Journal* for November, 1862, I gave some notes on the arrangements of the *genitalia*, &c. of most of our Orchids of the Northern States of the genus *Platanthera* or *Habenaria*. One common species, which was not met with last summer in season, I have now glanced at, viz.,

Platanthera pava, or *Habenaria flava* Gray. This, although ascertained by me to be the *Orchis flava* of Linnæus, so ill deserves its specific name, which I restored to it, (the flowers being in fact green, instead of yellow) that, notwithstanding priority, one would like to see it take Muhlenberg's name of *virescens*. This might well enough be allowed on the ground that the Linnæan name is a "*nomen falsum*."

As respects its arrangements for fertilization, I had anticipated that this would be an interesting species, on account of the strong protuberance or crest on the base of its labellum. This narrow and nasiform protuberance projects upwards and backwards, so as almost to touch the column between the two disks or glands of the stigma (or rather between the two cups or deep grooves which contain them), and therefore lying over and dividing the orifice of the spur. The anther cells are parallel, but set at a little distance apart: they lie almost in line with the labellum, but with the front ends depressed, so that the disks are a little lower than the base of the protuberance. These disks and this protuberance are so correlated in shape and position, that the proboscis of an insect fitted

to suck out nectar from the spur, inserted, as it must be, obliquely from above, cannot keep the median line at the entrance, but will take the right or the left of the protuberance, as may happen, and so will slide into the disk-bearing groove of that side. The structure of the disk-bearing portion of the column answers, perhaps, to what is expressed by Lindley's vague character of *Gymnadenia*, "*rostello complicato*," and is quite different from that which prevails in the more genuine species of *Platanthera*. But nearly every species has its peculiar arrangement. Viewed from the front (on removing the labellum), each disk is found to line an oblong cavity or deep groove: viewed vertically from above, this appears as a ring with the front edge cut away, or as something more than a semicircle, lined by the thin broad disk. On directing a delicate bristle vertically from above into the spur, taking either side of the protuberance of the labellum, the bristle will either enter the discal groove from above, as a thread enters the eye of a needle, or, if presented more obliquely from the front, will slide into the groove when, as it enters the spur, it is raised, as it must be, to a more vertical position. The disk clasps the bristle, adhering by its sticky surface, and is withdrawn with it along with the attached pollinium. No good observations were made as to any movement of the stalk of the pollinium on the disk when thus extricated, nor as to its application to the stigma.

It is evident that in this species self-fertilization cannot occur, that only one pollen-mass will be likely to be extracted at one visit of an insect, and that this will doubtless be conveyed to another flower to impregnate its stigma.

Gymnadenia tridentata, Lindl.—Examinations of flower-buds and open flowers, July 27–30, substantially confirm those of the previous year, which are recorded in this *Journal*, vol. xxxiv, p. 426, and in a foot-note on p. 260. The flowers before expansion are horizontal and somewhat reclining, so that the packets of pollen, which spontaneously detach themselves from the pollen-mass, may fall out when the anther-cells open. The anthers dehisce before the flower-bud is full grown, or at least four or five days before the flower opens. In every instance when the flower has naturally opened, the anther-cells will be found widely gaping, and several or many pollen-packets will be found upon the three "stigmatic processes," into which their pollen-tubes will have copiously and deeply penetrated. This, indeed, is the case two or three days before the blossom would have opened.

These three processes are so remarkably developed in this species, and they so strikingly represent functionally, and to appearance morphologically, three elongated clavate stigmas, that the species would not be regarded as a congener of *G. conopsea*, the type of Brown's genus *Gymnadenia*. *G. flava* Lindl., and *G. nivea* Lindl., which I have examined only in dried specimens, however, present intermediate states. In *G. flava*, there are two strong stigmatic lobes projecting laterally beyond the disks by the side of the base of each anther-cell. The middle or rostellar lobe is hidden by the approximate anther-cells, and is functionless. In *G. nivea*, the rostellar lobe is minute and hidden; the conspicuous lateral lobes are linear and deflected forwards so as to lie along the border of the base of the labellum on each side. I do not know whether they

receive the pollen, or whether there is a stigmatic surface between them under the disks, or both. But neither of these two species show any evidence of being self-fertilized in the bud. In *G. tridentata*, the species now in hand, the three elongated and somewhat clavate stigmatic bodies or processes, which are nearly alike, ascend, one on the outside of each anther-cell, and one between the two cells, to considerably above the level of the almost horizontal anther; their surface is loosely cellular and slightly viscid, so that the pollen-packets stick to them readily: all three, as already remarked, act functionally as stigmas. But also, underneath the disks and the common origin of the three stigmatic processes, I find a green surface, in position and character just like the stigma in *Orchis* and *Platanthera* except that (so far as I have observed, in unexpanded or freshly expanded flowers) it is only very slightly viscid. On application, few or no pollen-packets are left sticking to it, while they stick in considerable numbers to the upper part of the "processes." From the appearance, thus far, I should suppose that this normal stigmatic surface had become functionless. But, on the other hand, the large disks are in perfect condition; in the expanded flowers they adhere to a bristle or other body, and are thus removed from the shallow cups in which they rest, bringing away the caudicle with the considerable portion of the pollen-packets which still remain attached: the caudicle effects a prompt movement of depression, and now, if the bristle be returned to its position at the entrance of the spur, the pollen-mass will strike this broad ordinary stigmatic surface. The examination of older flowers may be expected to settle the question. But it is certain that the three linear club-shaped bodies act as stigmas.

In a systematic point of view, it is evident that the *Ophrydeæ* with naked disks need to be studied and arranged anew, upon living plants. But the forms cannot be clearly described and correlated until the morphology and terminology of the parts of the column have been reconsidered and elucidated. A. G.

ZOOLOGY.—

7. *Monograph of the Aye-Aye* (*Chiromys Madagascariensis* Cuvier); by RICHARD OWEN, D.C.L., F.R.S., &c.—The curious animal which forms the subject of this monograph, was first noticed in Madagascar by Souverat in 1780, and owes its name to an exclamation of astonishment uttered by the natives of the east coast, to whom, it is said, he exhibited it for the first time. Souverat brought home with him a stuffed skin and a cranium, which have since remained in the museum of the Garden of Plants, the only representatives of the species in European cabinets. Zoologists have been puzzled as to the true affinities of the Aye-Aye, some placing it among the Rodents, and others among the Quadrumana. Buffon assigned it a position in the former group, and in so doing was followed by Cuvier, who at the same time distinctly stated that it "is related to the Quadrumana in more points than one." In view of these differences of opinion, it is easy to see how desirable whole specimens were.

Science is indebted to the Hon. H. Sandwith, M.D., for the specimen, preserved in spirits, which forms the subject of the present investigation. This has enabled Prof. Owen to determine definitely its position among

the Lemuridæ, and at the same time add another to the admirable series of monographs by which the great English anatomist has contributed so largely to the progress of zoology and physiology in our day.

However remarkable the mingling of Rodent and Quadrumanous characters may be in the Aye-Aye, they are surpassed in the correlations of physical structure and strange habits. "The wide openings of the eyelids, the large cornea and expansile iris, the subglobular lens and tapetum, are arrangements for admitting to the retina and absorbing the utmost amount of light which may pervade the forest, at sunset, dawn, or moonlight. Thus the Aye-Aye is able to guide itself among the branches in quest of its hidden food. To detect this, however, another sense had need to be developed to great perfection. The large ears are to catch and concentrate, and the large acoustic nerve and its ministering 'flocculus' seem designed to appreciate any feeble vibration that might reach the tympanum from the recess in the hard timber, through which the wood-boring larva may be tunneling its way by repeated scoopings and scrapings of its hard mandibles." The food of this nocturnal animal, to whose strange physiognomy the eyes and ears add so much, consists mostly, as was ascertained by Souverat, of wood-boring grubs. To extract these, there are, united with the common Lemurine characters, chizel-shaped incisors, resembling those of Rodents, and a most remarkable modification of the middle finger, which is not only used for eliciting by percussion the hollow sound from the bored limb, but as a hook for extracting the grub. All the fingers are of somewhat unusual length, but the middle one "has been ordained to grow in length, but not in thickness with the other digits; it remains slender as a probe, and is provided at the end with a small pad and a hook-like claw." The use made of this part will be best learned from the very interesting letter to Prof. Owen by Dr. Sandwith, in which his own observations on the habits of the Aye-Aye are recorded.

Mauritius, Jan. 27, 1859.

My dear Mr. Owen:

After very great difficulty and much delay, I have at length obtained a fine male healthy adult Aye-Aye, and he is enjoying himself in a large cage which I have had constructed for him. . . . . I observe that he is sensitive of cold, and likes to cover himself up in a piece of flannel, although the thermometer is now often 90° in the shade. . . . . Now as he attacked every night the wood-work of his cage, which I was gradually lining with tin, I bethought myself of tying some sticks over the wood-work so that he might gnaw these instead. I had previously put in some large branches for him to gnaw upon; but the others were straight sticks to cover over the wood-work of his cage, which alone he attacked. It so happened that those I now put into his cage were bored in all directions by a large and destructive grub called the *Montouk*. Just at sunset, the Aye Aye crept from under his blanket, yawned, stretched, and betook himself to his tree, where his movements are lively and graceful, though by no means so quick as those of a squirrel. Presently he came to one of the worm-eaten branches, which he began to examine most attentively; and, bending forward his ears and applying his nose close to the bark, he rapidly tapped the surface with the curious second digit, as a woodpecker taps a tree, though with much less noise, from time to time inserting the end of the slender finger into the worm-holes as a surgeon would a probe. At length he came to a part of a branch which evidently gave out an interesting sound, for he began to tear it with his strong teeth. He rapidly stripped off the bark, cut into the wood, and exposed the nest of a grub, which he daintily picked out of its bed with the slender tapping finger, and conveyed the luscious morsel to his mouth.

I watched these proceedings with intense interest, and was much struck with the marvellous adaptation of the creature to its habits, shown by his acute hearing, which enables him aptly to distinguish the different tones emitted from the wood by his gentle tapping; his evidently acute sense of smell, aiding him in his search; his secure footsteps on the slender branches, to which he firmly clung with his quadrumanous members; his strong rodent teeth, enabling him to tear through the wood; and lastly, by the curious slender finger, unlike that of any other animal, and which he used alternately as a pleximeter (percuteur?), a probe, and a scoop.

But I was yet to learn another peculiarity. I gave him water to drink in a saucer, on which he stretched out a hand, dipped a finger into it, and drew it obliquely through his open mouth; this he repeated so rapidly that the water seemed to flow into his mouth. After a while he lapped like a cat; but his first mode of drinking appeared to me to be his way of reaching water in the deep clefts of the trees.

I am told that the Aye-Aye is an object of veneration at Madagascar, and that if any native touches one he is sure to die within a year, hence the difficulty of obtaining a specimen. I overcame this scruple by a reward of £10. . . . . .

Believe me, yours very faithfully,

H. Sandwith.

The "Conclusion" of the memoir will certainly attract very general attention, since it contains a distinct expression of Prof. Owen's view with regard to the great question of the day, that of "the origin of species." Those who have joined in the issue involved in this question may be arranged in one of two classes: 1st, comprising those who maintain that the present condition of the animal and vegetable kingdom was reached by a series of progressive creations, each species being created and suddenly introduced upon the surface of the earth, and the first formed individuals having the same specific characters as all the successors; 2d, those who deny the preceding view, and assert that all animals and plants are the result of "progressive development," "derivation" or "transmutation" of species, the first created forms being of the simplest kind, or at all events of a simpler kind than those of the present day, and in the course of time transformed into them. How the changes from simple to complex forms were effected, or how specific characters were modified, has been very differently explained. Lamarck says by a "*besoin*," Darwin by "natural selection" and "the struggle for existence," and Owen "by the ordained potentiality of second causes," and by transmutation "under law."

We do not propose, in this bibliographical notice, to enter into a discussion of these different theories, but, before citing Prof. Owen's views, we will merely remark that, if the progressive-creation hypothesis is adopted, we should be glad to see a better answer than has yet been made to the question, how and in what condition did the first forms make their appearance? When a mammal was created, did the oxygen, hydrogen, nitrogen, and carbon of the air, and the lime, soda, phosphorus, potash, water, &c., from the earth, come together and on the instant combine into a completely formed horse, lion, elephant, or other animal? If this question is answered in the affirmative, it will be easily seen that the answer is entirely opposed by the observed analogies of nature. In the practical study of the history of the earth and the changes which it has undergone, of the development of individual animals and plants, the "order of nature" points in one direction, viz: to the process of differentiation. The one-celled plant and the tree, the

polyp and man, and all organic forms intermediate between these extremes pass from the homogeneous to the heterogeneous, from the nucleated cell, or even from what is more simple still, from plasma to the adult individual consisting of organs more or less complex, according to the position in the series. We nowhere see plants or animals reach maturity in any other way than by development or growth.

At the same time, we must not lose sight of the fact that what is true of the successive stages of individual organisms may not necessarily prove true with regard to the history of the races; that while, from the earliest embryonic condition of each individual to the last, there is a connected series of observed changes or differentiations, and no break in the organic continuity, there are no observations whatever to prove a like organic continuity in the races. In the absence of such direct proof, we have no other alternative than to look to the analogies of nature and the geological record. The direction in which the former point is obvious; the testimony of the latter is thus far negative, but is it complete enough to be a safe guide?

In view of the difficulties met with in explaining the first introduction of living forms, Agassiz has put forth the hypothesis of the creation of eggs. "I then would ask, is it probable that the circumstances under which animals and plants originated for the first time can be much simpler or even as simple as the conditions necessary for their reproduction only, after they have been once created? Preliminary then to their first appearance, conditions necessary for their growth must have been provided for; for, if, as I believe, they were created as eggs, the conditions must have been conformable to those in which the living representatives first introduced now reproduce themselves. If it were observed that they originated in a more advanced stage of life, the difficulty would be still greater, as a moment's consideration cannot fail to show, especially if it is remembered how complicated the structure of some of the animals was, who are known to have been among the first inhabitants of our globe."—(*Contrib. Nat. Hist. of U. States*, i, 12.)

This hypothesis would answer very well for spawning fishes and reptiles, whose eggs may be trusted to the effects of physical agents. But does it help us with regard to viviparous fishes, viviparous reptiles, and mammals? To take the case of the mammals, what "conditions conformable to those in which the living representatives first introduced now reproduce themselves" would answer the purpose for the development of the young, except an uterus, or something analogous to an uterus, and for its nourishment after birth, except a mammary gland or something analogous to one? And how could there be an uterus or a mammary gland without organs of nourishment, locomotion, &c.; in other words, before creating the egg, it would be necessary to create some kind of an organism for the egg to live in. If such organism offered the same conditions with those of the individuals now living, why create the egg at all? Rather than this, it would seem to be a simpler matter to create the whole animal capable of producing eggs to begin with. If it be asserted that the conditions were not the same, this assertion would seem to be equivalent to the admission of variation,

inasmuch as the first egg would be capable of being developed under different circumstances from the later ones.

How Prof. Owen meets this difficulty with regard to the first introduction of species may be inferred from the following passages quoted from the monograph on the Aye-Aye.

"But the conception of the origin of species by a continuously operative secondary cause or law is one thing; the knowledge of the nature and mode of operation of that law is another thing. One physiologist may accept, another refute or reject, a transmutational or natural-selective hypothesis, and both may equally hold the idea of the successive coming-in of species by law."

"What I have termed the 'derivative hypothesis' of organisms, for example, holds that there are coming into being, by aggregation of organic atoms, at all times and in all places, under the simplest unicellular condition, with differences of character as many as are the various circumstances, conditions, and combinations of the causes educing them,—one form appearing in mud at the bottom of the ocean, another in the pond or the heath, a third in the sawdust of the cellar, a fourth on the surface of the mountain rock, &c., but all by the combination and arrangement of organic atoms through forces and conditions acting according to predetermined law. The disposition to vary in form and structure, according to the variation of surrounding conditions, is greatest in these first formed beings; and from them, or such as them, are and have been derived all other and higher forms of organisms on this planet. And thus it is that we now find, energizing in fair proportions, every grade of organization from man to the monad." . . . .

"Now the foregoing hypothesis is at present based on so narrow and, as regards the origin of life, so uncertain a foundation of ascertained facts, that it can be regarded only as a kind of vantage-ground artificially raised to expand the view of the outlooker for the road to truth, and perhaps as supporting sign-posts directing where that road may most likely be fallen in with." . . . .

. . . "And herein is one main distinction between it (origin of species by natural selection) and the 'derivative hypothesis,' which maintains that single-celled organisms, so diversified as to be relegated to distinct orders and classes of *Protozoa*, are now, as heretofore, in course of creation or formation, by the ordained potentiality of second causes; with innate capacities of variation and development, giving rise in a long course of generations to such differentiated beings as may be distinguished by the term 'plant' and 'animal;' from which all higher animals and plants have, through like influences, ascended and are being ascensively derived. This, as the naturalist knows, is mere hypothesis, at present destitute of proof. But it is more consistent with the phenomena of life about us, with the ever-recurring appearance of mould and monads, and with the coexistence, at the present time, of all grades of life rising therefrom up to man, than is the notion of the origin of life which is propounded in Mr. Darwin's book, 'On the Origin of Species by Natural Selection.'" . . . . .

"That organic species are the result of still operating powers and influences is probable from the great paleontological fact of the succes-

sion of such so-called species from their first appearance in the oldest fossiliferous strata; it is more probable from the kind and degree of similitude between the species that succeeds and the species that disappears never to return as such; the similitude being, in the main, of a nature expressed by the terms of 'progressive departure from a general to a special type.' Creation by law is suggested by the many instances of retention of structures in Paleozoic species, which are embryonal and transitory in later species of the same order or class; and the suggestion acquires force by considering the analogies which the transitory embryonal stages in the higher species bear to the mature forms of the lower species. Every new instance of structures which does not obviously and without straining receive a teleological explanation, especially the great series of anatomical facts expressed by the 'law of vegetative or irrelative repetition,'—all congenital varieties, deformities, monstrosities—opposes itself to the hypothesis of the origin of species by a primary or immediate and never repeated act of adaptive construction."

If we correctly understand Prof. Owen's views, as expressed in the above paragraphs, he inclines to, in fact adopts, though cautiously, the hypothesis of the origin of species by "transmutation" or "deviation;" these transmutations being in no accordance with a pre-arranged plan, but carried out under the influence of second causes. The first organisms were unicellular, brought into existence by spontaneous generation "under law," and, by a slow and orderly transmutation, ascensively differentiated into the highest vegetable and animal organisms. For the precise mode of bringing about the individual changes, he offers no conjecture, whatever.

We leave it for the advocates of progressive creation to answer these views—and will conclude with expressing the belief, that there is no just ground for taking, and that we arrive at no reasonable theory which takes, a position intermediate between the two extremes. We must either assume, on the one hand, that living organisms commenced their existence fully formed, and by processes not in accordance with the usual order of nature, as it is revealed to human minds, or, on the other hand, that each species become such by progressive development or transmutation; that, as in the individual so in the aggregate of races, the simple forms were not only the precursors, but the progenitors of the complex ones, and that thus the order of nature, as commonly manifest in her works, was maintained. J. W.

8. *On the "Minute Vertebrate Lower Jaw."*—In the *Annals and Magazine of Natural History*, for October, 1862, Dr. Wallich has given a figure and description of a jaw-like object $\frac{1}{100}$ of an inch in length, dredged at St. Helena, which he considers as "evidence of the existence of a vertebrate animal measuring only $\frac{1}{20}$ inch in length!" This has excited much discussion, several papers having since been written upon the subject, and although its vertebrate character has been fully disproved, there is much diversity of opinion in regard to the true character of the object. C. Spence Bate (*Ann. and Mag.*, Dec., 1862) thinks it to be the claw of an Amphipod. It has also been suggested that it may be part of the lingual ribbon of a Gasteropod; or part of the manducatory apparatus of a Rotifer. Mr. Busk, in an illustrated paper

in the *Quarterly Journal of Microscopical Science*, for Jan., 1863, has given the most probable solution:—that the jaw figured by Dr. Wallich is one of the valves or jaws of a pedicellaria of an Echinoderm, allied to *Amphidotus*.

In connection with this subject of the pseudomorphs of vertebrate jaws, we may take occasion to mention the jaws of some Annelids, the resemblance of which to those of the higher animals has not been mentioned by any of Dr. Wallich's critics. In the mouth of the proboscis of worms of the family *Aphroditacea*, such as *Polynoe*, *Sigalion*, and *Acoëtes*, we find upper and lower jaws, each composed of two rami, and armed with strong tooth-like processes. In seizing prey, these jaws work upon each other vertically, as in fishes and reptiles,—in contradistinction from the lateral action which takes place in all other Articulates, and which has been stated to be universal in that sub-kingdom. In the cut, fig. *a* represents the lower jaw of *Acoëtes lupina*, seen from above, the rami being pressed outward a little, to show their inner surfaces. Fig. *b*, a lateral view of the exterior surface of one of the rami. In the strong pair of teeth representing the canines and some other points, we have certainly as much resemblance to a vertebrate lower jaw as in the object originally figured by Dr. Wallich. W. S.

9. *Note on the Megatherium;* by Prof. Agassiz, (*Proc. Bost. Soc. Nat. Hist.*, ix, 193.)—Prof. Agassiz, remarking on the Megatherium cast, presented by Mr. Bates, which had recently been mounted at the Museum of Comparative Zoology, stated that he regarded the position given it, which is very nearly that of Owen, as not quite accurate; and that the Megatherium, instead of being set up erect, should have been placed in a crouching attitude, with the hind legs bent, sufficiently so that the tail should touch the ground,—with the head bent down between the front legs, the broad chest resting upon the ground, supported by the fore-legs, extended in such a way that they should rest for nearly their whole length, and leave simply a free play for the extremities to reach out beyond the head.

V. ASTRONOMY AND METEOROLOGY.

1. *Procession and Periodicity of the November Star-shower.*—In the last number of this Journal (p. 148) it was suggested that the November star-shower has a motion along the *sidereal* year of one day in seventy years. The following dates, taken from the tables in the last number of the Journal, show this motion, and also indicate that the shower has a period of about a third of a century, as Prof. Olmsted, Prof. Twining, and Mr. Herrick have supposed. In the last column is given the date obtained by adding to the corresponding day of 1850 one day for each 70 years from the time of the shower to 1850.

A.D. 1833,	Nov.	12·7	corr. to	A.D. 1850,	Nov.	13·3,	becoming	Nov.	13·5
1799,	"	11·6	"	"	"	12·9,	"	"	13·6
1698,	"	8·6	"	"	"	11·6,	"	"	13·8
1533,	"	3	"	"	"	7·0,	"	"	11·5
1366,	Oct.	29·5	"	"	"	5·6,	"	"	12·5
1202,	"	26	"	"	"	4·1,	"	"	13·4

A.D. 1101,	Oct. 24	corr. to	A.D. 1850,	Nov.	3·0,	becoming	Nov.	13·7
1002,	" 20	"	"	Oct.	31·1,	"	"	12·2
934,	" 19	"	"	"	31·5,	"	"	13·8
931,	" 19	"	"	"	31·3,	"	"	13·4
903,	" 18·5	"	"	"	30·0,	"	"	12·5

The last date is given in the previous table as A.D. 902, Oct. 29th or 30th. A partial examination of the historical evidence leads me to the probable conclusion that the true date is the night of Oct. 18–19th, A.D. 903.

To comprehend fully the force of the argument involved in this table, the original records of these several showers should be given. This procession seems to imply that the orbit of the body furnishing these meteors has only a small inclination to the ecliptic, and that the motion is retrograde. The small distance of the radiant from the point to which the earth is moving, to wit 7°, confirms this conclusion. H. A. N.

2. *Star shower in* 1606.—On a bright night, Nov. 15th, 1606, it seemed as though it rained stars; first fell only the largest and brightest stars from heaven, then indiscriminately the large and the small ones in great numbers. Before they reached the earth they were extinguished.—E. A. Bielz, *Verhandlungen des Siebenbürg. Vereins zu Hermannstadt.*, Jahrg. xiii, 1862. Quoted in *Pogg. Ann.*, cxviii, 496.

3. *Meteorite of Tucson.*—The meteoric iron of Tucson, described on pp. 153–154 of this volume, and called on p. 154 the Bartlett meteorite, in allusion to its being figured and described by Mr. Bartlett in his Report, was first brought to the notice of the scientific world by Dr. John L. LeConte, as mentioned in this *Journal*, vol. xiv, 2nd ser., p. 289, (March, 1852).

4. *Observations of the August Meteors.*—The Committee upon Periodical Meteors of the Connecticut Academy of Arts and Sciences have this year undertaken a more extended system of observations than in any year preceding. In accordance with the desire and action of the Academy, a stellar chart suited to observations at all times was prepared by Prof. H. A. Newton, of the Committee, and distributed to observers at various stations, together with instructions for observing at the August period. The plan was to have parties of observers at two principal stations, New Haven and Hartford. These parties were to communicate by telegraph, so as to identify the meteors and to enable each corps of observers to give especial attention to those which had been actually seen at the other station. It was also an important part of the plan to secure the coöperation of other persons, especially of such as were near enough to see the meteors observed at Hartford and New Haven. Four hours on each of the three nights, from Aug. 8th to Aug. 11th, were designated as hours for watching. Notwithstanding the very unfavorable state of the weather, especially on two of the nights, and the failure of the telegraph also on a part of the third night, the Committee feel that they have been successful in their efforts. A goodly number of paths have been observed with such care as to afford data for computing their parallax. Data also for obtaining some idea of the velocities of these bodies have been obtained.

Two of the Committee, Prof. Elias Loomis and the undersigned, took charge of the party at the New Haven station. The remaining two

members, Prof. C. S. Lyman and Prof. H. A. Newton, directed that at Hartford.

Observations have been received from Prof. Bache at the Coast Survey station near Wolcottville, Conn., from Prof. Hopkins at Williams College, from Mr. Hough, Director of the Dudley Observatory at Albany, from Mr. B. V. Marsh at Philadelphia, and from various observers at other stations, as will appear more fully from the report of Prof. Newton given below; who has undertaken the collation and reduction of the observations.

It appears that the meteors were most abundant on the morning of the 11th.

For the Committee,

ALEX. C. TWINING, *Chairman.*

New Haven, Aug. 17, 1863.

Summary of observations of shooting stars during the August period, 1863, compiled by H. A. NEWTON.

July 25th.—From $9^h\ 30^m$ to 10^h P. M., I saw six shooting stars. There was a bright moon.

Aug. 3d.—From 2^h to 4^h A. M., Dr. A. W. Wright and myself recorded 16 paths. The sky was clear and moon bright. We could just see δ Ursae Minoris. One eighth of the time was lost in recording. The same day from $9^h\ 30^m$ to $10^h\ 30^m$ P. M., I saw only four flights. The moon had just risen.

Aug. 5th.—In the evening in ten minutes two of us saw four meteors.

Aug. 7th.—From $9^h\ 30^m$ to $10^h\ 45^m$ P. M., I saw 17 flights. The time actually employed in looking was estimated at 50 minutes. The sky was very clear and without moon.

Dr. A. W. Wright observed with a comet seeker, the diameter of whose field was about $2\frac{3}{4}°$, and whose magnifying power was 16, for a little more than an hour, commencing at 10 o'clock. He saw four shooting stars, three of which were not much brighter than the smallest stars visible through the instrument. He looked towards Polaris.

Aug. 8th.—The committee had proposed to make concerted observations from 9^h P. M. till 1^h A. M. But the clouds intercepted all view of the heavens at most places, and interfered seriously at the others.

At New Haven, Prof. Twining, Prof. Loomis, Dr. Wright, Mr. Robert Brown, Jr., Mr. W. Stocking, Mr. J. H. Kerr and Mr. T. W. Twining formed the observing party. They saw about 20 meteors and recorded most of their paths.

At Hartford were Prof. Lyman, Mr. P. H. Woodward, Mr. C. G. Rockwood, Mr. T. Hooker and myself. We recorded on the chart over 20 paths, and described in a general manner about 10 more.

At Hamden, 4 miles from New Haven, Prof. W. D. Whitney secured four observations.

At Philadelphia, Mr. B. V. Marsh, looking towards the northeast, recorded 14 paths, which were nearly all that he saw in three hours.

Mr. W. H. Hale, Ph.D., at Albany, saw two meteors, but the clouds prevented farther watching.

At Easton, Pa., Prof. J. H. Coffin saw and recorded 14 paths.

At least five shooting stars were observed at two places so as to furnish their parallax. Three of them were seen at three different places.

Two or three others were seen at two places, but not well enough to obtain altitudes.

Dr. Wright, looking with the comet seeker towards Polaris, saw, in about an hour of time suited for observing, four shooting stars. Two were nearly as bright as Polaris. The others were much fainter.

Aug. 9–10*th.*—The clouds and rain prevented observation at most places. We had arranged to observe from 11^h P. M. until 3^h A. M. Soon after one o'clock the clouds broke away partially at Hartford and New Haven. We obtained about 10 correspondences between these two places. Hon. J. Hammond Trumbull assisted us in Hartford. There were more shooting stars to be seen than on the previous night.

Prof. Whitney at Hamden, Mr. Marsh at Philadelphia, Mr. C. M. Whittelsey at Colchester, Conn., and Dr. Hale at Albany, each recorded several flights.

Aug. 10*th*–11*th.*—The sky was clear at some places but not at others.

At New Haven were Prof. Twining, Mr. Brown, Mr. Stocking, and Mr. T. W. Twining. Dr. Wright was using the comet seeker. At Hartford, Prof. Lyman, Mr. Trumbull, Mr. Allen, Mr. Rockwood, Mr. Hooker and myself observed.

During the first hour and a half, the telegraph noted about 20 coincident observations, when one of the operators was taken ill. The flights were so frequent that some of these may be found to be coincidences of time only. The sky at New Haven soon after clouded over, and the party broke up. At Hartford, about 150 paths were recorded and many others described but not drawn on the chart.

Dr. Wright had about a half hour of good time for observing, in which he saw four very faint meteors.

At a station of the U. S. Coast Survey near Wolcottville, Prof. Bache and his assistants had made very careful preparations for observing. The weather was however very unfavorable. Prof. Bache reports 16 paths recorded by Mr. Geo. W. Dean, Assistant U. S. Coast Survey, Mr. R. E. Halter, Sub-assistant, and Mr. S. H. Lyman, Aid.

At Williamstown, Prof. Hopkins, assisted by Mr. Edward W. Morley, who was formerly an assistant in the observatory, recorded about 40 paths. The times of appearance were kept by an assistant at the transit clock.

Mr. Hough, aided by Mr. Simons and Mr. McClure, made at the Dudley Observatory careful estimates of the duration of flight, and a considerable number of determinations of place of appearance and disappearance.

Mr. R. Norman Foster, at Northampton, Mass., reports places of more than 20 paths, the times of appearance being noted by Mr. Louis Lamporte. Assistance was given him by Mr. Orlando Hastings and Mr. N. S. Wiard.

From Hamden, Prof. Whitney reports about 70 paths seen between 10^h P. M. and 3^h A. M.

Dr. Hale sends from Albany about 40 paths observed.

Mr. C. M. Whittelsey, at Colchester, Conn., recorded over 40 flights between 9^h and 12^h P. M.

Rev. T. S. Potwin, at East Windsor, Conn., and Rev. Wilder Smith at Berlin, Conn., each recorded about ten paths.

J. H. Worrall, Ph.D., at Newark, Delaware, recorded about 25 paths. Mr. Marsh, at Philadelphia, and Mr. F. Bradley, at Chicago, also made similar observations.

The numbers of shooting stars seen on this night were much larger than on the previous night. We think they were a little larger than on previous years.

Mr. B. V. Marsh observed alone near Philadelphia 130 flights between 10^h P. M. and 1^h 50^m A. M. He counted as conformable all which radiated from Perseus or Cassiopeia.

From	10^h 0^m	to	10^h 15^m,	in	15	minutes	he saw	2	conformable,	1	unconformable.
"	10 15	"	10 30	"	15	"	"	6	"	1	"
"	10 30	"	10 45	"	15	"	"	7	"	0	"
"	10 45	"	11 00	"	15	"	"	4	"	0	"
"	11 0	"	11 15	"	15	"	"	6	"	0	"
"	11 15	"	11 30	"	15	"	"	7	"	4	"
"	11 30	"	11 45	"	15	"	"	12	"	1	"
"	11 45	"	12 0	"	15	"	"	4	"	1	"
"	12 0	"	12 15	"	15	"	"	9	"	2	"
"	12 34	"	12 45	"	11	"	"	10	"	1	"
"	12 52	"	1 0	"	8	"	"	5	"	1	"
"	1 0	"	1 15	"	15	"	"	7	"	0	"
"	1 15	"	1 30	"	15	"	"	17	"	2	"
"	1 30	"	1 45	"	15	"	"	14	"	0	"
"	1 45	"	1 50	"	5	"	"	3	"	3	"
	Total,			in	204	minutes,		113	conf'mable,	17	unconformable.

There was spent in locating tracks, making notes, &c., probably one-fifth of the time, say 41 minutes. This reduces the time actually engaged in watching for meteors to 163 minutes, making the hourly number for one observer about 48.

At Manchester, Me., Mr. Joseph G. Pinkham and Mr. E. Pope Sampson counted in three hours 257, as follows:

From	9^h	to	10^h,	44	conformable, and	2	unconformable.
"	10	"	11	57	"	4	"
"	12	"	1	142	"	8	"
Total in 3 hours,				243	conformable,	14	unconformable.

At Williamstown, a party of about a dozen counted 162 between 11 and 12 o'clock.

At Hartford, six of us, counting aloud to prevent duplication, saw—

From	3^h	10^m	to	3^h	20^m,	A. M.,	55 flights.
"	3	20	"	3	30	"	49 "
"	3	30	"	3	40	"	49 "
Total in a half hour,							153 "

Many of these were seen by two or more persons. It was estimated that the average number seen by each one during the half hour was about 40. The moon, the clouds near the horizon, and the twilight somewhat diminished the number. It was estimated that there was no increase of frequency over the two hours preceding. About a dozen were unconformable.

At Northampton, two persons saw 43 meteors between 9^h and 10^h 30^m. One person, between 10^h 30^m P. M. and 2^h A. M., assisted by a second observer during the last ten minutes, saw 360.

Capt. C. E. Dutton, near Portsmouth, Va., counted 38 in about an hour, early in the evening.

A party of three persons at the house of Prof. Silliman, Jr., counted 96 meteors from $9^h\ 15^m$ to $10^h\ 15^m$, about fifteen per cent of them were unconformable. A fourth person was present one third of the time.

Mr. Francis Bradley, at Chicago, had made arrangements for observing throughout the period. The clouds covered the sky until near one o'clock on the morning of the 11th. From $1^h\ 45^m$ to 2^h A. M. he counted 14 flights. From 1^h to 2^h he saw 87, of which 14 were not conformable to the radiant.

Mr. Francis Miller, at Sandy Spring P. O., Md., with an assistant, observed during the three nights, but the results have not yet reached us.

It was noticed at several places that almost the only very brilliant flights were early in the evening. This appears to have been connected with the fact that the radiant was then near the horizon.

Prof. Twining has given especial attention to the determination of the duration of flight of the meteors. He employed the method described by him in a previous number of this Journal ([2], vol. xxxii, p. 448). The average of over 20 estimates is about $0^s{\cdot}6$, and the average angular velocity $14\frac{1}{2}°$. This he considers too great for the average duration of flight of *all* the shooting stars, which is not probably more than $0^s{\cdot}4$ or $0^s{\cdot}5$. His estimates of time include the instant between the first perception and first definite sight. The space moved over in this instant not being usually included in the arc as laid down, the angular velocity above given is probably too small. The discussion of the estimated times of flight and velocities will be considered more at length hereafter. Valuable materials for this purpose have been contributed by various observers.

Prof. Twining watched on the evening of Aug. 11th from $11^h\ 45^m$ to 1^h A. M., for an exact determination of the radiant. This he locates at R. A., 47° 45′, N. P. D., 32° 45′. The radiant for the preceding evening he places near the sword handle of Perseus. At $10^h\ 25^m$ of that evening he saw a stationary star (the fourth he had ever seen) at R. A., 35°, N. P. D., 31° 40′. He considers the motion of the radiant, pointed out in a previous article (this Journal, [2], xxxii, 444), confirmed by the appearances this year.

I feel great doubt however myself of the motion of the radiant. The radiant is not a point, but a region of some size. Its exact determination is to be made out, I think, only by considering a large number of paths. The stationary star seen by Prof. Twining of course fixes only the direction of that path. Mr. Allen, at Hartford, at $2^h\ 18^m$ A. M., saw a stationary star in the constellation Triangulum. If the exact shape and size of the region of emanation could be determined, it would give us valuable materials for finding the shape and extent of the ring, or disk, which furnishes these small bodies.

At Natick, Mass., Mr. F. W. Russell, assisted by Mr. J. H. Wilson, Mr. F. W. Harwood and Mr. E. H. Wolcott, observed on various evenings from Aug. 4th to Aug. 13th. The following facts are condensed from his report.

Aug. 4th.—Three observers while walking in the evening saw in one hour 12 meteors.

Aug. 5th.—From 9^h to 10^h P. M., two persons saw, while walking, 15 flights, 10 of them in the last 15 minutes.

Aug. 6th.—Three observers saw 30 meteors in an hour. A faint aurora visible a part of the time.

Aug. 7th.—Sky much obscured. In ten minutes two persons saw 5 flights.

Aug. 9th.—A friend saw 20 very bright meteors in less than 30 minutes.

Aug. 10th.—In the morning, from 3^h 40^m to 4^h, Mr. Russell saw 13 flights, twelve conformable. In the evening, four observers watched from a little after 9^h P. M. until a little after 12 P. M., and three observers the rest of the time until 2 o'clock A. M. The following is the result:

From	9^h	to	10^h,	74	conformable,	2	unconformable,
"	10	"	11	123	"	9	"
"	11	"	12	140	"	24	"
"	12	"	1	124	"	21	"
"	1	"	2	119	"	25	"
		Total,		580	"	81	" in all 661.

No two meteors were counted twice. The hourly averages from the above table are, for each observer, 1st hour 19, 2d hour 33, 3d hour 41, 4th hour 46, and 5th hour 48. While arranging the watch, 27 more were seen, making 688 in about 5^h 10^m. The radiant was in the triangle, η, γ, τ Persei. Seven meteors were observed which passed through Cassiopeia almost into this triangle.

Mr. W. G. Bryant, at Winchendon, Mass., saw 41, between 8^h 30^m and 10^h P. M.

Aug. 11th.—In the evening, Mr. Russell saw—

From	9^h	to	10^h,	6	conformable,	4	unconformable.
"	10	"	11	29	"	10	"

Thin clouds and distant lightning interfered with observation.

Aug. 12th.—Two observers, from 9^h to 11^h P. M., saw 67 meteors, as follows:

From	9^h	to	10^h,	23	conformable,	11	unconformable.
"	10	"	11	18	"	15	"

Aug. 13th.—Two observers, from 9^h to 10^h P. M., saw 12 conformable, and 11 unconformable.

Mr. Russell gives the following positions of the centre of the radiant:

Aug.	4th,	R. A.,	7°	0′,	N. P. D.,	35°	30′,
"	5th,	"	358	20	"	31	10,
"	6th,	"	20	15	"	31	10,
"	7th,	"	27	40	"	33	45,
"	10th, A. M.,	"	43	0	"	37	0,
"	10th–11th,	"	39	35	"	36	20,
"	11th, P. M.,	"	42	30	"	37	40,
"	12th,	"	43	20	"	38	10,
"	13th,	"	49	25	"	31	20.

On some of these days, he was not able to fix satisfactorily the position, yet he thinks none will vary more than one degree from the truth, and that most of them are exact within half of a degree.

VI. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Electric Illumination at Boston,—Photometrical powers of the light,* (in a letter to Prof. B. SILLIMAN, Jr., from Prof. WILLIAM B. ROGERS, dated Boston, August 14, 1863).—I send you a short account of my observations on the power of the electric light exhibited with such striking effect by Mr. Ritchie on the evening of the 6th inst. (the National 'Thanksgiving') in this city. Through his kindness, I was enabled to make a more satisfactory measurement of the illuminating force of the battery at the State House, than I had the means of doing on the occasion of its former exhibition on the 4th of July.

The battery in question, consisting of 250 Bunsen elements, having each an acting zinc surface of about 85 inches, and grouped in five batallions of 50 each, was arranged in the dome of the State House, and the carbon light and the photometric apparatus prepared for the purpose were placed in line across the same apartment, commanding a range of about fifty feet.

In view of the immense power of the light as observed in the previous experiment, I substituted for the 20-candle gas burner, used at that time as the standard of comparison, a unit ten times as great, formed by the flame of a kerosene lamp placed in the focus of a small parabolic reflector and throwing its concentrated light on a photometric screen of prepared paper fixed in front of it, at the distance of five feet. Before the observation, the lamp and reflector were so adjusted as to make the light cast on the near side of the screen equivalent by measure to the action of 200 candles.

This was done by the intervention of a kerosene lamp fitted up with a bridge of platinum wire for defining and restricting the height of the square flame. Such a lamp I find of frequent use in ordinary photometry, as, when suitably adjusted, it gives the light of about eight standard candles, and thus transfers the measurement in the photometer to the wider divisions of the scale. Being suspended in a balance of peculiar construction, its rate of consumption enables us to correct for any slight departure from the assigned illumination. The lamp thus regulated was placed with its flat flame 12 inches from the screen, while the lamp in the reflector was distant 60 inches—and the flame of the latter was adjusted until the effects on the screen were equalized.

A platform supporting the standard lamp and screen at the assigned distance was arranged to slide on a horizontal graduated bar, extending directly towards the carbon points so that the screen should receive the rays from the electric light and from the reflector perpendicularly on its opposite faces. In making the observations, the platform was moved to and fro until the illumination on the opposite sides of the screen was judged to be equal, and then the measured distances of the two antagonizing lights from the screen gave by easy computation their relative illuminating power.

By a series of such observations, it was found that the carbon light had a force varying from 52 to 61 times that of the lamp with reflector, making it equivalent in illuminating power to the action of from 10,000 to 12,000 standard sperm candles pouring their light from the same

distance upon the surface of the screen. This it will be remembered is the effect of the unaided carbon-light sending its rays equally in all directions from the luminous centre, and falls vastly short of the illuminating force of the cone of collected rays which was seen stretching like the tail of a comet from the surface of the great reflector. Judging from some recent experiments on the power of such a reflector to augment the intensity of the light emanating from its focus, there can be no doubt that, along the axes of the cone when brought to its narrowest limits, the illuminating force of the carbon light as displayed on the State House could be rivalled only by that of several millions of candles shining unitedly along the same line.

In the above described observations, a thick screen was necessary on account of the great intensity of the lights to be antagonized. I need hardly say that the different color of the two lights added much to the difficulty of the measurements. But, by marking in each case the extreme limits on either side, it was practicable to adjust the screen pretty accurately to equality of illumination.

The only previous experiment of precisely the same kind which I can recall is that of Bunsen, cited in the books, which was made with a battery of 48 elements. In this the photometric equivalent of the carbon light was estimated at 572 candles, or nearly 12 candles to the cell. My observations show a power more than three times as great, or about 40 candles to the cell, a difference due no doubt largely to the more intensive battery at my disposal and the cumulative effect of its arrangement. I suspect too that the elements in Bunsen's observation were of inferior size, but on this point I am without definite information.

2. *Vermilion Rock Salt Mine at Petite Anse, Louisiana.*—One of the facts of scientific interest brought to light by the Southern rebellion is the discovery of an important deposit of rock salt of remarkable purity in the island of Petite Anse, in Vermillion Bay, on the Gulf coast of Louisiana. By the kindness of Mr. Geo. D. Colburn, a large specimen of this salt has reached us. Its analysis by Dr. J. L. Riddell, of New Orleans, gives the following composition: Chlorid of sodium 98·88, sulphate of lime 0·76, chlorid of magnesium 0·23, chlorid of calcium 0·13, =100. This analysis, it will be seen, makes the Petite Anse salt almost pure.

Salt springs had been known on the island from an early period, but no suspicion existed of there being rock salt near the surface until the late owner, Judge Avery, with the view to improve the flow of water from one of the saline springs, caused an excavation to be made, when, at the moderate depth of only fourteen or fifteen feet from the surface, the laborers struck the bed of white rock salt, which they at first imagined must be ice. It was at once recognized, and proved of incalculable advantage to the Confederates, as well as a source of great wealth to the owner. The Island of Petite Anse (Little Elbow Island), so named from its shape, is a body of very fertile land, supporting rich crops of sugar cane and corn, besides forest trees, about 2½ miles long by 1½ wide and containing about 2100 acres. It rises 170 feet above tide in the midst of a wide spreading marine swamp. The soil of the island is an amber-colored argillaceous sandy loam, capable of forming good bricks.

The salt is covered by a whitish, cream-colored solid smooth rock, at

an average depth of about 19½ feet below the surface or 4½ feet below tide water. There is no moisture or brine in the deposit, the salt being compact, hard and perfectly dry. Our intelligent correspondent is not a geologist, nor does he send us any fossils with the salt. But the deposit is undoubtedly of Tertiary age.

3. *Note on the Rule of Priority.*—In reading the reclamation of Mr. Crookes, reproduced in the March No. of this Journal (pp. 277–279), it seems that some confusion prevails, through which a rule of naturalists, regulating nomenclature, is misapplied. Mr. Lamy is said to state, and Mr. Crookes cites it as if indisputable, that "*it is priority of publication which constitutes priority of invention;*" and the latter thinks himself thereby precluded from producing even the personal testimony of an observer in his behalf. We venture to say that the rule above-cited has no existence, and from the nature of the case could and ought to have none. The fact of a *discovery* is to be established by evidence, and no sort of evidence by which it may be established can be excluded. Abundant illustrations of this may be adduced from the history of almost every science. The rule which has been here misapprehended is one which fixes *nomenclature.* Naturalists have established, and physicists, &c., have adopted, the very necessary rule that publication is essential to give validity to a *name,*—that the name first published takes precedence. The discovery of a fact or a thing, and the imposition of a name, are two different matters, and not rarely dissociated. The first is to be established by any good evidence: the second is governed by an arbitrary but most just rule. Thus the name *Thallium,* it appears, is established by Mr. Crookes, by priority of publication. The date of the discovery of the metal, to which this name is given, is to be authenticated by whatever testimony can be adduced,—is a question of fact and not a question of nomenclature. A. G.

[The preceding 'Note' was prepared immediately after the issue of the March number of this Journal. As it states clearly a well established though often forgotten principle in the ethics of science, we now put it on record for future reference. In the June number of the *Phil. Mag.*, Mr. Crookes has published a full history of the discovery of thallium with all its documents, for an early copy of which he has our thanks. Tested by the criterion of the above Note, all doubt on the priority of Mr. Crookes's discovery disappears, if any before existed. It is not requisite to discuss whether the printing of labels and the exposure of specimens in the great Exhibition of 1862 constituted publication. The question of *priority* is decided in Mr. Crookes's favor by abundant testimony which no weight of academic or personal influence can overthrow.]

4. *The thirty-third Meeting of the British Association for the Advancement of Science* was held at Newcastle-upon-Tyne, Aug. 26, but at this time, of course, no report of the proceedings has reached us.

5. *Personal.*—Dr. Wolcott Gibbs, one of the associate Editors of this Journal, has accepted his election to the Rumford foundation in Harvard College, left vacant some time since by the resignation of Prof. E. N. Horsford. Dr. Gibbs enters on his new duties at Cambridge at once, having already removed from New York, where he has so long and ably discharged the duties of the chair of Chemistry and Physics in the New York Free Academy.

VII. BOOK NOTICES.

1. *Heat considered as a mode of Motion:* being a course of twelve lectures delivered at the Royal Institution of Great Britain in the season of 1862; by JOHN TYNDALL, F.R.S., &c., Prof. of Nat. Phil. in the Royal Institution. With illustrations. New York: D. Appleton & Co. 1863. 12mo, pp. 480.—This book is destined to become a classic in the literature of science. With all the skill which has made Faraday the master, in Great Britain, of experimental science, Dr. Tyndall enjoys the advantages of a superior general culture, and is thus enabled to set forth his philosophy with all the graces of eloquence and the power of superior diction. The Royal Institution is truly fortunate in the succession of the eminent men who, from its organization at Sir Joseph Banks's house on the 9th of March, 1799, to this time, have made its name illustrious. Young, Davy, Faraday, Tyndall, in physical science, Smith (Sir James Edward), Lindley, Roget, Grant, Jones, Carpenter, Huxley, and Owen, in natural history and anatomy, form a list such as few literary or scientific institutions in the world can boast.

The lectures at the Royal Institution have always been of commanding interest. In them Davy first expounded the philosophy of flame, the nature of chlorine, the existence of the alkali metals. Faraday followed with his ever-memorable electrical researches, the laws of electrolysis, the conductibility of gases, diamagnetism, and the magnetism of gases; Tyndall, on the transmission of heat through gases, the glaciers and vegetation, and now with the course before us, in which, with a simplicity and absence of technicalities which render his explanations lucid to unscientific minds, and at the same time a thoroughness and originality by which he instructs the most learned, he unfolds all the modern philosophy of heat, commencing with the researches of von Mayer fifteen years ago, and embracing those of Joule and others at a later date. The first seven lectures deal with *thermometric heat:* its generation and consumption in mechanical processes; the determination of the mechanical equivalent of heat; the conception of heat as molecular motion; the application of this conception to the solid, liquid, and gaseous forms of matter; to expansion and combustion; to specific and latent heat; and to calorific conduction.

The remaining five lectures treat of *radiant heat:* the interstellar medium, and the propagation of motion through this medium; the relations of radiant heat to ordinary matter in its several states of aggregation; terrestrial, lunar, and solar radiation; the constitution of the sun; the possible sources of his energy; the relation of this energy to terrestrial forces and to vegetable and animal life.

The author rises to the level of these questions from a basis so elementary, that a person possessing any imaginative faculty and power of concentration can accompany him.

Our readers will have an opportunity of becoming familiar with many of the fundamental ideas developed so beautifully in Dr. Tyndall's book, by perusing the memoir of von Mayer, the republication of which is commenced in the present number. It will need no addition of ours to induce all who feel an interest in this department of research to read up by Dr. Tyndall's "Heat as a mode of Motion."

2. *Brande and Taylor's Chemistry.*[1]—This book is remarkable among English manuals of chemistry for entering directly upon the discussion of pure chemistry without the usual chapters on general and chemical physics. Matter and its properties, crystallization, isomorphism, chemical affinity, solution, electrolysis, equivalent weights and volumes, and nomenclature and notation, are all disposed of in about seventy pages. The body of the book (609 pages) is almost equally divided between the subjects metalloids, metals, and organic chemistry. It is eminently a practical old-fashioned book, prepared by authors who are neither of them remarkable for their own additions to the science, but who have a well-earned reputation as teachers and compilers, functions which, when ably discharged, are certainly most meritorious. This volume is very carefully prepared, and abounds on every page with marks of fidelity and patient reading. It is an excellent book to put into the hands of students, and as a guide to the teacher, although its uniform small type, unrelieved by any change of appearance beyond italic heads, and destitute entirely of all illustrative cuts, renders it by no means easy reading. It is impossible to prepare a book on chemistry more completely in contrast with the recent work of Dr. Odling, (the first part which we owe to the kindness of the author) in which the unitary system is, for the first time in English, thoroughly carried out.

3. *Supplement to Ure's Dictionary.*[2]—Dr. Ure's dictionary has a hold upon the general mind beyond that of any similar work in the English language. It is a very acceptable thing, therefore, to a large class of readers, to have this Supplement by Mr. Hunt, designed to bring matters up to the present time in very numerous departments in which the rapid progress of science and art had left very much to desire.

Assisted by about fifty of the best men in England, Mr. Hunt has produced in this Supplement a work certainly of varied, but in the main of great excellence. While it is truly a Supplement to Ure's Dictionary, it is also an independent and very comprehensive book of reference by itself, furnished at a moderate cost and consequently acceptable to a large number. In the new English edition of Ure in three volumes (at a cost of $38) the materials of this American edition are distributed throughout the older matter. The American edition is also in three volumes (embracing 3212 pages), of which this Supplement is the third.

4. *The American Annual Cyclopedia and Register of Important Events of the year* 1862. Vol. II. New York: D. Appleton & Co. Large 8vo, pp. 830.—This work is designed to embrace a double character, as an annual register and as a continued supplement to the American Cyclopedia by the same editors, the completion of which we have already announced (this Journal, [2], xxxv, 304). As is both natural and

[1] Chemistry. By William Thomas Brande, D.C.L., F.R.S., L. and E., and Alfred Swaine Taylor, M.D., F.R.S., &c. Philadelphia: Blanchard & Lea. 8vo, pp. 696.

[2] A Supplement to Ure's Dictionary of Arts, Manufactures, and Mines, &c., edited by Robert Hunt, F.R.S., F.L.S., Keeper of Mining Records, assisted by numerous contributors eminent in science and familiar with manufactures. Illustrated with 700 engravings on wood. New York: Appleton & Co. 1863. Large 8vo, pp. 1096.

proper, a large space is devoted to the registration of the great political events which are now in progress in the United States. Thus, under "Army operations," we have a review of military events for 1862, filling 172 pages, with numerous maps and plans. But science is not forgotten under the heads Astronomy, Auroras, Barometer, Building materials, Chemistry, Earth, Electricity, Illumination, Meteorology, and many other titles, which are partly reviews of progress and partly new additions. The work is very valuable as a book of reference, and displays great industry and good judgment in its preparation.

5. *Transactions of the Illinois Natural History Society*, Vol. I, 2nd ed. 194 pp. 8vo. Springfield, Illinois; edited by C. D. Wilder, Secretary.—This first part of volume I. contains a short paper on a geological section of the Rock river valley, Ill., by O. Everett, M.D.; another on the remains of the Mastodon in Illinois, by C. D. Wilder; others containing catalogues of Illinois species in Botany and some departments of Zoology, by R. H. Holder, C. Thomas, and G. Vasey, besides an article on Insects injurious to vegetation, by B. D. Walsh. The Mastodon remains described were found in Northern Illinois near Aurora (west of Chicago). They consist of two tusks and seven teeth. The tusks were 10 feet long.

6. *On the Origin of Species, or the Causes of the Phenomena of Organic Nature*—a course of six Lectures to Working Men; by Thomas H. Huxley, F.R.S., etc. 150 pp. 8vo. 1863. New York, D. Appleton & Company.—This little volume, republished from the English edition, is a brief popular discussion of the great subjects mentioned in the title. We have already expressed some of our objections to the teachings of the author on species, in our notice of his work on "Man's Place in Nature."

OBITUARY.

Dr. Samuel Prescott Hildreth, of Marietta, Ohio, for nearly forty years a constant contributor to this Journal, died July 24, 1863, at Marietta, after an illness of about three weeks. His disease, enteric fever, commenced July 5, resulting in hemiplegia on the 18th, manifested by insensibility and paralysis of the right side, which continued until death. He was nearly 80 years of age, having been born Sept. 30th, 1783, in Methuen, Essex Co., Massachusetts.

Dr. Hildreth was one of the first pioneers of science in the country west of the Alleghany Mountains. His first communication appeared in the tenth volume of this Journal, (1826), "On facts relating to certain parts of Ohio." His series of 'Meteorological registers' commenced in vol. xvi, for 1828, and has been continued without intermission for thirty-five consecutive years, the last being published in March of this year (xxxv, 181). At the request of the Senior editor of this Journal, Dr. Hildreth undertook an exploration of the coal regions of the Ohio, which was published in January, 1836, (vol. xxix, p. 1), under the title of "Observations on the Bituminous coal deposits of the valley of the Ohio, and the accompanying rock strata; with notices of the fossil organic remains and the relics of vegetable and animal bodies, illustrated by a geological map, by numerous drawings of plants and shells, and by views of interesting scenery." This was the most important of Dr. Hildreth's scientific labors and by far the most valuable contribution which up to that time had appeared on the subject discussed. It filled an entire number

(155 pages) of this Journal, and was profusely illustrated by figures of fossils, sections, and original drawings, embraced in thirty-six plates on wood.

In 1837, Dr. Hildreth was appointed one of the assistant Geologists upon the Geological Survey of the State of Ohio; his report forms part of the published documents relating to that survey. The memory of Dr. Hildreth will always be cherished among the early contributors to American geology.

Dr. Hildreth was also an industrious and acute observer of facts in his special department, and the medical journals from 1808 to 1825 contain many valuable papers from his pen.

His active mind embraced among the objects of his research various interesting historical questions, and we are indebted to his industry and personal knowledge for the preservation of many valuable facts relating to the early history of Ohio and Western Virginia. Such are his "*History of the Settlement of Belville, Western Virginia,*" published in the *Hesperian Magazine.* In 1848 he published "*Pioneer History of the Ohio Valley and of the Northwest Territory,*" a volume of 525 pages. This work is drawn chiefly from original sources, and is full both of entertainment and instruction. It contains plans and picturesque views of the early forts and villages of the pioneer settlers, who were emigrants from New England and chiefly from Connecticut. In 1852, he issued his "*Lives of the Early Settlers of Ohio,*" an 8vo. of 539 pages.

His collections in various departments of natural history, to the number of about 4000 specimens, he presented, together with his scientific library, to Marietta College, Ohio, where they occupy a room known as the 'Hildreth Cabinet.'

Dr. Hildreth did not shrink from his share of the duties and responsibilities of civil life which the republic imposes on all her sons. At the age of 27 we find him (in 1810 and again in 1811) in the Ohio Legislature as a supporter of Jefferson and Madison. He held the office of collector of non-resident taxes for eight years, when the office (in 1819) was abolished. He was also Clerk of the Ministerial Lands, to the close of his life, for 53 years.

In his private life he illustrated every virtue of a christian gentleman. Bright and cheerful by nature, he loved nature with the simple enthusiasm of a child. Industrious and systematic in a high degree, no moment of his life was wasted. In his family, we have seen a beautiful example of domestic happiness and warm hearted hospitality. He lived with nature and nature's God—and among the patrons and co-workers in this Journal who have left its founder almost alone, no one has shed a purer and more mellow light in the horizon of his setting sun—no one has departed more loved and regretted by the Senior Editor.

Joseph Stillman Hubbard, Professor of Mathematics in the U. S. Navy, and since 1845 detailed to duty in the Naval Observatory at Washington, died at the house of his widowed mother, in New Haven, Aug. 16th, 1863, aged 40 years.

Few of the younger men of science in America have a more honorable reputation than Prof. Hubbard. His taste for astronomy and his

mathematical ability were evident during his undergraduate course at Yale College, where he graduated in 1843. Early in 1844, he was appointed an assistant in the High School Observatory, at Philadelphia, then in charge of the distinguished astronomer, Sears C. Walker. The next autumn he was employed by Captain (now Major General) Fremont to reduce his Rocky Mountain observations, and was invited to accompany him on his next expedition. Declining this offer, he was, at the instance of Fremont and Senator Benton, appointed by Hon. Geo. Bancroft, then Secretary of the Navy, a Professor of Mathematics in the U. S. Navy, and assigned to duty in the then new Naval Observatory, at Washington. This post he filled with distinguished zeal and fidelity to the time of his death. The printed volumes of the Washington observations are full of the evidences of his skill as an observer and a computer. His powers, under the peculiar management of Superintendent Maury, were made to contribute, perhaps, more to the factitious reputation of that person, than to his own advantage. The flight of Maury to his own place, and the accession of Gilliss to the head of the Observatory, was no less a matter of congratulation to American science than to the officers of the Observatory.

Prof. Hubbard was a frequent contributor to the *Astronomical Journal.* His investigations on Biela's comet are there recorded in a series of elaborate papers, as also those on the great comet of 1843, on the orbit of Egeria and many others. The article, Telescope, in Appleton's *American Cyclopædia*, a valued paper of much labor, is also from his pen.

His labors of love in the cause of benevolence and religion were not less zealous and unremitting in the discharge of every duty, than in the paths of science. He was married in 1848, but his wife died before him. He left no children.

James Renwick Chilton, of New York, long known as a leading commercial chemist, died at Yonkers, N. Y., July 24, 1863, at the age of 54 years.

Stillman Masterman, of Weld, Maine, died July 19, 1863, aged 32 years. He was an esteemed astronomical observer and an occasional correspondent of this Journal. A notice of his life and labors will appear in our next.

Memoirs of the American Academy of Arts and Sciences, New Series, vol. viii, part i.—1, An unpublished Grammatical Fragment: Petronius Arbiter de Antiquis Dictionibus; *Charles Beck.*—27, On the Alloys of Copper and Zinc; *Frank H. Storer.*—57, On the Impurities of Commercial Zinc, with special reference to the Residue insoluble in dilute acids, to Sulphur, and to Arsenic; *Charles W. Eliot* and *Frank H. Storer.*—97, Remarks on the latest form of the Development Theory; *Francis Bowen.*—123, On the Secular Variations and Mutual Relations of the Orbits of the Asteroids; *Simon Newcomb.*—153, Plantæ Wrightianæ e Cuba Orientali (Polypetalæ et Apetalæ); *A. Grisebach.*—193, Filices Wrightianæ et Fendlerianæ, —nempe in Insula Cuba a Carolo Wright et in Venezuela ab Aug. Fendler, ann. 1854–60 (nonnullis Panamensibus etc. interjectis), Enumeratæ curæ *Daniel C. Eaton, A.M.*—221, On the light of the Moon and of the planet Jupiter; *George P. Bond.* —287, Comparison of the light of the Sun and Moon; *G. P. Bond.*—299, A Catalogue of the declinations of 532 Stars culminating near the zenith of the Observatory of Harvard College, Cambridge; *T. H. Safford.*—333, The Lumbar Enlargement of the Spinal Cord; *John Dean, M.D.*, with four plates.—354, On some of the relations of Salts of Zinc and Alumina to Soda and Potassa; *E. N. Horsford.*